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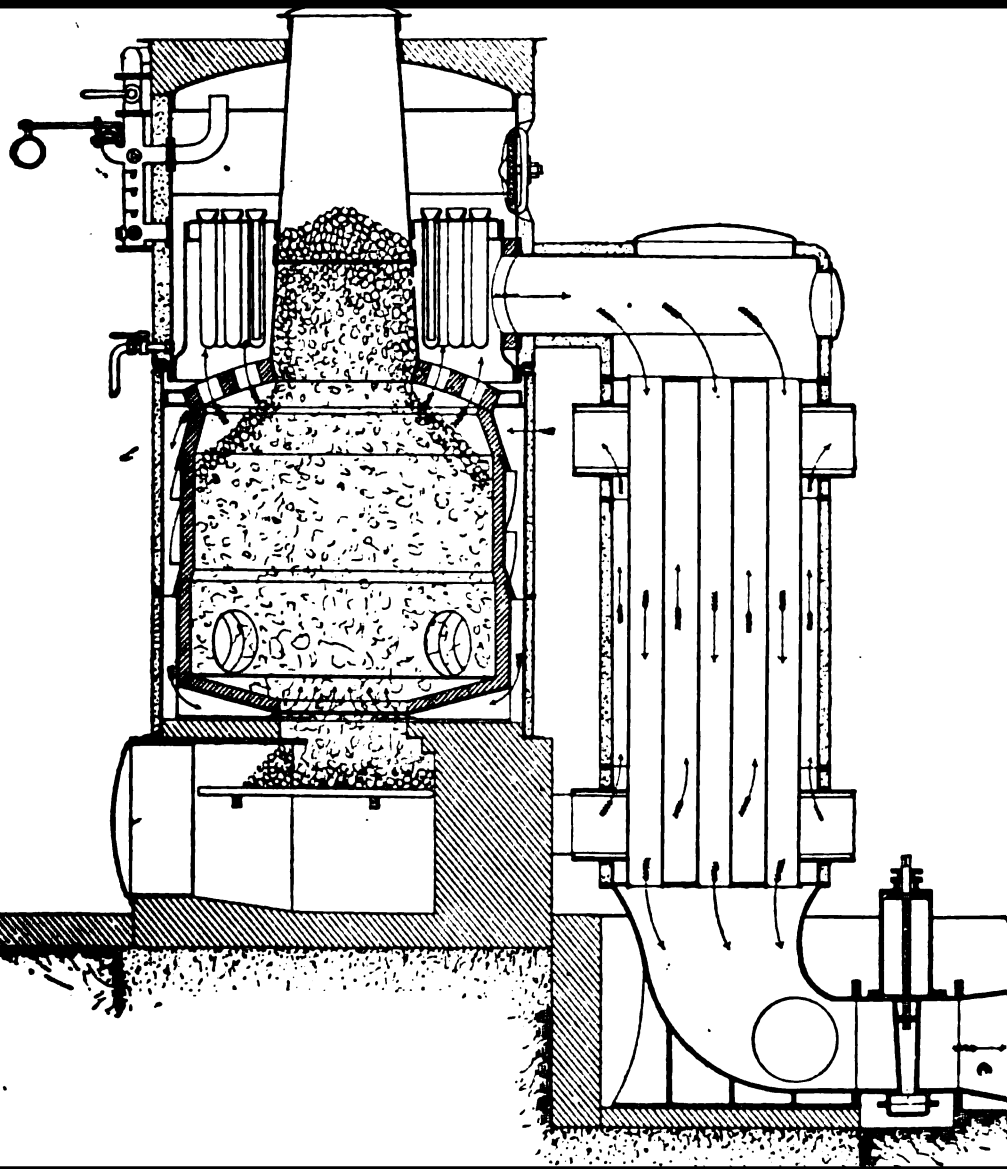

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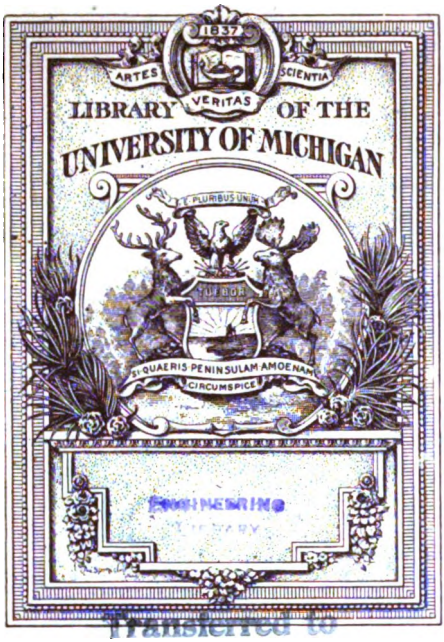

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MINUTES OF PROCEEDINGS

OF

THE INSTITUTION

OF

CIVIL ENGINEERS;

WITH OTHER

SELECTED AND ABSTRACTED PAPERS.

VOL. LXXXIV.

EDITED BY

JAMES FORREST, ASSOC. INST. C.E., SECRETARY.

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CORRIGENDA.

Vol. lxxxi., p. 227, line 8 from bottom, *for* $(1 + 0{\cdot}0021\,t)$, *read* $(1 - 0{\cdot}0021\,t)$.
„ lxxxii., p. 384, line 20 from bottom, *for* "Elle Halle," *read* "Hati-ella."
„ lxxxiii., p. 444, line 3 from bottom, *for* "causes," *read* "careers."

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THE

INSTITUTION

OF

CIVIL ENGINEERS.

SESSION 1885-86.—PART II.

SECT. I.—MINUTES OF PROCEEDINGS.

12 January, 1886.

Sir FREDERICK J. BRAMWELL, F.R.S., President, in the Chair.

The following Associate Members have been transferred to the class of

Members.

WILLIAM ARMITAGE BROWN.
THOMAS PENN GASKELL.
GEORGE ANDREW HOBSON.
WALTER PITT.
CHARLES EDWARD ROBINSON.
WILLIAM HENRY SCOTT.
HENRY FRANCIS SNEYD-KYNNERSLEY.
WILLIAM HERON STEEL.
LEWIS GORDON TYRELL.

The following Candidates have been admitted as

Students.

JOHN GEORGE BARKLEY.
ARTHUR HOWARD BENHAM.
JAMES BRACEGIRDLE.
CHARLES ROBERT BRADY.
ERNEST CANNING.
ALFRED CHATTERTON, B.Sc.
THOMAS CECIL DANIELL.
WALTER JOHN EASTON.
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HAROLD MEDWAY MARTIN, Wh.Sc.
WALTER EDWARD MAY.
JOHN CLARKSON PHILLIPS MAYNARD.
PHILIP WILLIAM RYGATE, B.A., B.E.
JAMES NASMYTH SIDEBOTHAM.

The following Candidates were balloted for and duly elected as

Honorary Member.

JEAN CHARLES ADOLPHE ALPHAND.

Members.

THE HON. EMMANUEL LEWIS GALIZIA. | THOMAS MONRO.
THOMAS FORTH ROTHERHAM.

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JAMES WILLIAM WELCH.
JULIUS WIBORG.

Associates.

HENRY DOULTON.
WILLIAM HUNTER, J.P., F.G.S.

(*Paper No. 2083.*)

"On Gas-Producers."

By FREDERICK JOHN ROWAN.

THE large degree of favour in which the use of gaseous-fuel is now held amongst those connected with manufacturing-operations is undoubtedly due, in great part, to the labours of the late Sir William Siemens. Yet large as that degree is, compared with the practice of twenty years ago, the system of gas-firing is still far short of a universal adoption; but it is not going too far to predict that, as the subject becomes more widely understood, the examples of any other method of treating fuel will become few and exceptional.

It must be acknowledged, that the use of gaseous-fuel results naturally from a clear perception of the principles of combustion. It is readily seen that the first stage in the combustion of all ordinary fuels is their conversion, in large measure, from the solid

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or liquid into the gaseous form, and that it is only when they reach this stage that their value for heating purposes can be properly realized. The distilling and vaporizing processes involved in that conversion absorb heat, and their character thus points to their being separated from subsequent heat-producing operations. When they are carried on in the same chamber in which the resulting gases are burned, the maximum temperature attained is, as might be expected, much lower than that which is attainable by other means. There are also sources of loss of heat inseparable from the direct use of solid fuel, such as imperfect combustion; the impossibility of minimizing the quantity of air introduced for combustion, on account of the complexity and constant variation in the operations involved; radiation from solid residue, &c., all of which intensify that result.

"The inferiority of the temperature which is made available in this manner," Mr. D. K. Clark, M. Inst. C.E., remarks, "is, for many applications of heat, of much more serious import than the actual loss of heat by quantity; and it has long been recognized that the only system of heating which carries with it a complete remedy for the shortcomings of the ordinary fire, and the imperfections of various fuels, is the system of heating by gases generated from the fuel, by a process resembling distillation, which, being collected and conducted to the region where the heat of combustion is to be utilized, are mixed with air, ignited, inflamed and consumed on the spot. These gaseous substances arriving without the accompaniment of ash or cinder, at the place for action, do not alter or affect the surfaces of the bodies destined to receive the heat; and the heat is discharged, by radiation as well as by conduction, just where it is required."[1]

The actual conditions of combustion and the extent of the various losses, in quantity of heat, occurring in coal-fired furnaces have been formulated by Rankine,[2] Minary,[3] Schwackhöfer[4] and others; while R. Galloway[5] has directed attention to the difference in temperature, or calorific intensity, obtained when carbon is burned in air, and when carbonic oxide is so burned.

It appears that a higher calorific intensity may be obtained

[1] "Fuel, its Combustion and Economy." By D. Kinnear Clark, 1879, p. 277.

[2] "A Manual of the Steam Engine, and other Prime Movers," &c.

[3] "Publication Industrielle des Machines, Outils et Appareils," vol. xviii. p. 195 *et seq.*

[4] "Fuel and Water," from the German of Franz Schwackhöfer, edited by W. R. Browne, p. 109.

[5] "A Treatise on Fuel," 1880.

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with the gaseous-fuel, chiefly on account of the smaller quantity of air required for combustion, and as a consequence the much lessened dilution of the heat by inert nitrogen and carbonic acid.

Thus when carbon is burned in air, 3 parts of carbon combine with 8 parts of oxygen to form 11 parts by weight of carbonic acid, the specific heat of which at constant pressure is 0·2164. These 8 parts of oxygen are associated with 26·8 parts of nitrogen in the air, the specific heat of nitrogen being 0·244.

$$\frac{3 \times 8{,}080}{11 \times 0{\cdot}2164 + 26{\cdot}8 \times 0{\cdot}244} = 2{,}717°{\cdot}6 \text{ C.}$$

When carbonic oxide is burned in air the following is the result:—

$$\frac{2{,}403}{1{\cdot}57 \times 0{\cdot}2164 + 1{\cdot}91 \times 0{\cdot}244} = 2{,}982° \text{ C.}$$

The investigations of the late Dr. Grouven[1] of Leipsic, of Mr. William Foster,[2] and of others, by directing attention to the nitrogen contained in coal, and to the large proportion remaining in coke and recoverable therefrom, have added to the advantages of the use of gaseous-fuel the important consideration of the value of by-products which may be associated with its production. If any of the nitrogen existing in coal is to be recovered as ammonia, the employment of gas-producers of some sort becomes a necessity.

Classification of Producers.—In the production of gas from coal and coke the following classes of apparatus have been devised and used:—

1. Retorts, heated externally, for distilling the hydro-carbons from coal apart from contact with air: coke, as a refuse material, being withdrawn periodically. This is the ordinary form of plant in use in gas-works.

2. Generators, or producers, in which coal is consumed or converted into gas by combustion with access of air. The hydro-carbons are distilled in these appliances by means of the heat conducted or radiated from the zone of combustion, and the coke or fixed carbon is burned with air first to carbonic acid, which is thereafter reduced to carbonic oxide by contact with the incandescent carbon.

3. Producers, in which the same result is arrived at, with the

[1] "Landwirthschaftliche Versuchsstationen," vol. xxviii.
[2] Minutes of Proceedings Inst. C.E., vol. lxxvii. p. 162.

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modification in the composition of the resulting gas due to admixture of steam with the air-supply.

In these two latter classes the producers may have either a grate with bars or a closed hearth, and may consist of one or of two chambers; in the latter case the distilling-chamber may be heated partially or entirely from flues or passages on the outside; the resulting gas is of the same composition in both cases, and must contain from 30 to about 60 per cent. by volume of nitrogen derived from the air introduced for combustion.

In one form of the apparatus of class 3 (viz., Young and Beilby's), steam is used in excess, while the combustion of the coke is maintained at a comparatively low heat, as the primary purpose of this apparatus is not the production of good heating-gas (these conditions not being favourable to that, and in fact a considerable quantity of carbonic acid is formed), but the securing of the nitrogen of the fuel in the form of ammonia.

4. Apparatus in which incandescent coke or anthracite is used to decompose steam for the production of "water-gas," either (1) continuously, by the use of retorts heated from the outside, which contain the carbon in contact with which steam is decomposed; or (2) intermittently, by the use of generating-chambers in which the carbon is first brought to a state of bright incandescence by an air-blast, and into which, secondly, steam is passed through the carbon, the air-blast being shut off; or the heat produced by the combustion of the carbon, or of gases from the generating-chamber, is, in the first instance, stored up in brickwork, which being made white-hot, is then used to decompose steam.

5. The Author has recently proposed an apparatus combining the retort system of distilling the hydro-carbons from coal, with means for consuming the resulting coke or fixed carbon in an atmosphere of steam only, excluding air and applying a sufficiently high temperature to prevent or minimize the formation of carbonic acid, the whole working together continuously and automatically.

The practical value of any example of these various classes depends upon the quality of the gas produced and the cost of production, a rough practical test of the efficiency of a gas-producer being afforded by the percentage of carbonic acid and nitrogen contained in the gas, and the volume of gas of a given temperature produced per ton of coal. An exception must be made in the case of such special apparatus as that of Young and Beilby, which manifestly must be tested by the yield of ammonia obtained per ton of coal.

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A Table containing, it is believed, all the trustworthy analyses of producer-gases which are available is given in the Appendix, so that a comparison of the different products may be made. These analyses show that the gas from producers worked by internal combustion does not contain more than 45 per cent. (but as low sometimes as 26 per cent.) of combustible ingredients, having, according to the various authorities, a theoretical calorific intensity of from 1,575° to about 2,200° Centigrade. Water-gas, made either by means of retorts or of intermittent apparatus, and ordinary illuminating-gas made by distilling coal, contain from 86 to 97 per cent. of combustible matter with corresponding possibilities of producing high temperatures. Illuminating-gas has, however, a higher calorific value than ordinary water-gas, but not higher than the gas made in the apparatus of Lowe and Strong.

The investigation of thermo-chemical data, connected with the question of the economical working of producers, does not enter into the scope of this Paper, but the advantage of using steam along with the air-supply in internal-combustion producers may be referred to.

Mr. R. Schöffel[1] has calculated (on the supposition that producers are worked at a minimum temperature of 1,200°) that 85 parts of carbon are gasified by air when 12 parts are gasified by steam, and that 18·5 parts of steam are required for 100 parts of carbon.

Mr. A. Wilson[2] has shown that the mixture to produce 100 parts by weight of gas is roughly:—coal, 17·5; air, 79·9; and steam, 4·0; or that 100 parts of coal require 22 of steam. Allowing for the difference between coal and carbon, Schöffel's calculation is practically in accordance with Wilson's, and from it the following is the theoretical composition of the gases when steam is used:—

Carbon monoxide	(85 + 12) 2·333 = 226·3	or 37·25	per cent.
Hydrogen	= 2·0	„ 0·33	„
Nitrogen. . . .	85 × 4·463 = 379·3	„ 62·42	„

When working with air alone the composition would be—

Carbon monoxide	(85 + 12) 2·333 = 226·3	or 34·33	per cent.
Nitrogen . .	(85 + 12) 4·463 = 432·9	„ 65·67	„

The amounts of heat yielded by an equal weight of carbon in the gases are by calculation—

[1] "Chemisches Centralblatt," vol. xv. pp. 414 to 431.

[2] The Journal of the Society of Chemical Industry, vol. ii. 1883, p. 458.

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With steam and air $226{\cdot}3 \times 2{,}400 + 2 \times 29{,}000 = 601{,}120$ calories.
„ air only . $226{\cdot}3 \times 2{,}400 = 543{,}120$ „

Difference 58,000

This difference turned into carbon $= \frac{58{,}000}{8{,}080} = 7{\cdot}17$, which, as 97 parts of carbon are used, shows an apparent saving of 6·9 per cent. when steam is used.

It is, however, probable that, in the future, efforts will be directed towards the perfection of means of producing gas free from the dilution of the large quantity of nitrogen derived from an air-supply, as this system not only affords the means of obtaining high temperatures of combustion, but also tends to simplify furnace arrangements, rendering it necessary to heat only the air prior to combustion in them, and not, as at present, both air and gas.

Historical notice.—The historical portion of the subject embraces the various steps in the introduction of gaseous-fuel; the numerous attempts to manufacture it according to the information possessed at the time, and the experience thus gained; and the different designs of producers, with the influence of the introduction of suitable furnaces for the use of gas upon these designs.

The employment of waste gases[1] from iron-furnaces and other metallurgical operations was clearly the first movement in this direction.

Lampadius[2] in 1801 proved the possibility of using the waste gases escaping in the carbonization of wood, and in 1809 to 1811 Aubertot employed the waste gases of blast-furnaces for roasting ores, burning lime, &c. It is stated[3] that a patent was granted to Aubertot in 1812, and in 1814 he suggested that suitable furnaces should be built for the employment of waste gases in metallurgical operations. These facts were communicated to the French Academy of Sciences, in a Report[4] by Messrs. Thenard, Berthier, and Chevreul, in May, 1842, Berthier[5] having published a paper on the subject in June, 1814.

---

[1] This is different from the utilization of waste heat. Dr. Percy mentions early patents for utilizing waste heat, viz.: Gardiner's, 1788; and Slater's, 1831. Percy's Metallurgy. Fuel, pp. 532, 533.

[2] "A Practical Treatise on Metallurgy." By W. Crookes and E. Rohrig, vol. iii. p. 433.

[3] Ronald and Richardson's Tech., vol. i.; Fuel, vol. i. p. 157.

[4] Annales des Mines, 3rd series, vol. xiii. p. 715.

[5] Journal des Mines, June, 1814, vol. xxxv. p. 375.

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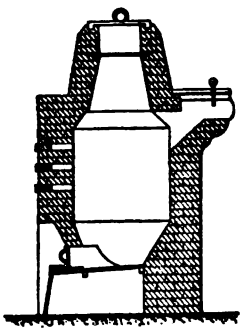
Lampadius[1] in 1830, at the Lead Smelting Works of Halsbrücke, near Freiberg, endeavoured to cupel silver-lead by means of coal-gas; and in May, 1836, according to a statement made by Ébelmen,[2] a patent was obtained by Victor Sire, of Clerval, for the manufacture of wrought-iron by means of waste gases from blast-furnaces. The patent became public in consequence of its not having been practically introduced within the prescribed time, but the Central Jury of the Paris Exhibition of 1844 reported[3] that it was employed in 1838 at the ironworks of Jägerthal on the Lower Rhine, and, since 1841, it has been in use in ironworks at Treveray, in the Department of the Moselle, for refining iron.

In 1832 a patent was taken out in England[4] for the utilization of the waste heat of blast-furnaces, but it is questionable whether this and perhaps some other early plans, for diverting "the flame heretofore discharged into the open air," were founded upon a correct idea of the actual problem.

In 1837, however, Wilhelm von Faber du Faur carried out some laborious and successful experiments, at Wasseralfingen in Würtemburg, in which he used the waste gases in puddling- and refining-furnaces. These experiments, it is admitted, went far to ensure the adoption of the plan of utilizing waste gases for combustion in other furnaces, and to direct attention generally to the advantages of gaseous-fuel.

FIG. 1.



BISCHOF PRODUCER.

Producer-gas, Bischof.—Before Faber du Faur's experiments had become widely known, Bischof experimented, in 1839, at Mägdesprung in the Hartz,[5] upon the production of combustible gases by means of a separate generator or producer. This producer (Fig. 1) was built of brick, and was of cylindrical shape with conical ends, the central cylindrical part being 5 feet in diameter and 5½ feet high; the upper conical part 4 feet 4 inches high, terminating in a charging-door; and the lower conical part, 2 feet deep, terminating in a grate under which was an ash-pit

[1] Percy's Metallurgy. Iron and Steel, p. 463.
[2] Annales des Mines, 4th series, vol. ii., p. 371.
[3] "A Practical Treatise on Metallurgy." By W. Crookes and E. Rohrig, vol. iii. p. 494.
[4] Percy's Metallurgy. Iron and Steel, p. 464.
[5] "A Practical Treatise on Metallurgy." By W. Crookes and E. Rohrig, vol. iii. p. 495.

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closed by an iron slab. A branch with a damper led the gases off at the base of the top conical part, and in the cylindrical body there were three holes at different levels, closed by brick stoppers, for observing the progress of combustion. Air was admitted through apertures in the slab closing the ash-pit, and was drawn in by the chimney-draught of the furnace or furnaces connected with the producer. No blast was used, and the gases when led off were mixed with air at the furnace. Gas thus produced from peat yielded the highest welding heat.

Bischof's results were not at once made use of, because attention was directed to Faber's experiments with waste gases, which it was thought offered a more economical source of gaseous-fuel. There were, however, some Austrian metallurgists[1] who visited Faber's works in 1838, and formed an adverse opinion of the practical value of his experiments, and who tried, in 1839 and 1840, at ironworks in Jenbach, in the Tyrol, to produce combustible gases by the imperfect combustion of small charcoal. Although the practicability of firing with artificially-produced gases and their good effect were demonstrated by these trials, the experiments were interrupted by severe explosions of the gas.

Karsten in 1841, and Heine in 1842, directed attention to the feasibility and advantage of such gas-producers as a means of making use of certain kinds of fuel, which were not suitable in their ordinary state for the production of high temperatures, and in both these years further trials were made.

Ébelmen, Thomas and Laurens[2] investigated the subject in France in 1838; and at the same time Frérejean made experiments at Crans, near Annecy in Savoy; while in 1842 experiments following up those made at Jenbach were carried out at the Steel Works, in St. Stephan in Styria, in which favourable results were obtained from gases made from small brown coal. These results were at once published, and gave rise to the more general introduction on the Continent of methods of artificially producing combustible gases.

Ébelmen.—A generator erected in October, 1840, at the ironworks of Audincourt in France, under Ébelmen's directions for his experiments, resembled a small blast-furnace (Fig. 2). It was worked with a blast of air, the introduction of which was subdivided as much as possible. In height inside this producer

[1] "A Practical Treatise on Metallurgy." By W. Crookes and E. Rohrig, vol. iii. p. 495.

[2] "Traité de la Chaleur." By E. Péclet. 2nd edition. Vol. i. pp. 273–283.

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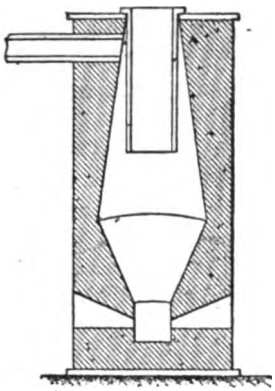
was 10 feet from hearth to throat, 3 feet 4 inches in diameter at the widest part above the boshes, and about 10 inches in diameter above the hearth. The throat was 13 inches in diameter. A cast-iron pipe descended from the throat for about 4½ feet into the body of the furnace, forming a shaft, or hopper, which was kept constantly full of fuel, a lid being used when necessary. Surrounding this pipe was a space in which gas collected, and from which it was led by a branch-pipe. The fuel used at Audincourt was small charcoal, and it was found that the blast caused a much more abundant supply of gas to be produced than with Bischof's plan, which had the additional disadvantage that the producer could not be placed on or above the level of the furnaces in which the gas was to be used. In the case of Ébelmen's producer the gas could be forced downwards by the blast-pressure for some distance, but the melting of the ash of the fuel under the great heat of combustion caused by the blast was found to interfere with the regular supply of gas. At first the melted ash adhered to the brick sides of the producer, and so obstructed the passage of air, but by the introduction of some fusible slag, or flux with the fuel, the ash was melted and run out at the bottom. Ébelmen's results obtained with this generator were communicated to the Academy of Sciences in 1842.[1]

Fig. 2.



Ébelmen Producer.

Ekman.—Another early form of gas-producer (which is illustrated in Percy's Metallurgy) was invented by Gustav Ekman of Sweden,[2] and was successfully used there prior to 1845, along with gas-furnaces which were also designed by the same ironmaster.

In the illustration (Figs. 3 and 4) the chamber for the production of the gaseous-fuel is built of firebrick, and enclosed within a jacket of cast-iron, a free annular space being left between the two. In the firebrick wall of the producer there are two rows of tuyeres, the upper one containing four, and the lower one three tuyeres. In the iron jacket opposite the tuyeres are small corresponding holes, fitted with movable plugs, by means of

[1] Comptes Rendus, vol. xiv. p. 174; and Fichet in Mémoires de la Société des Ingénieurs Civils, 1874, p. 678.

[2] Jern-Kontorets Annaler, 1850; Percy's Metallurgy, Fuel, p. 518.

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which the interior of the producer may be inspected while working. A pipe in the iron jacket admits cold air at a pressure of about 1 inch of mercury from a blower into the space. The blast in its passage through this space becomes heated to from 90° to 150° Centigrade, the jacket and the wall of the producer being kept cool by this absorption of the heat. On the top of the producer is a hopper through which fuel is supplied by means of a sliding bottom worked by a lever. An opening for the removal of ashes is fitted with a door. The producer communicates with the body of the furnace at the firebridge. In the roof of the furnace, on the producer side of this firebridge, is a series of openings, connected above with an iron box, which communicates with the blast space by two iron pipes provided with stopcocks. By this arrangement a portion of the air entering through the blast-pipe passes into the box, from which it descends through the openings referred to. When the producer is filled with ignited fuel, and air is blown in through the pipe, carbonic oxide is copiously produced, which in its way towards the firebridge is met by descending jets of heated air entering through the openings, and is thereby effectually burned.

Fig. 3.

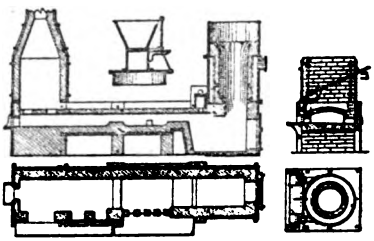
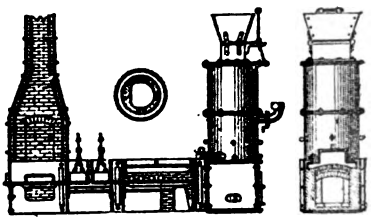


Fig. 4.



Ekman Producer.

Wood charcoal was the fuel generally employed in this producer, and a very high degree of heat was obtained by its use. Peat, artificially dried but not charred, was also extensively used in such producers in Sweden. Caking coal is an unsuitable fuel for this producer, but anthracite might, as Dr. Percy suggests, be employed provided that steam (by preference superheated) were injected along with the air in order to keep down the temperature in the body of the producer.

Beaufumé.—In the year 1856 an ingenious "gasifier," the invention of Mr. Beaufumé, was introduced and tried by the French Government at the Imperial Arsenal at Cherbourg. The results of the trials, which were said to have been very satisfactory, were, with a description of the apparatus, communicated by the late

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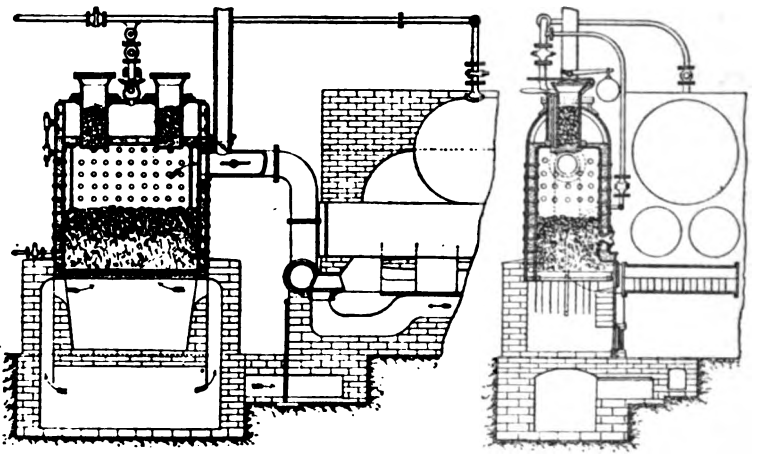
Professor Macquorn Rankine, to the Institution of Engineers in Scotland in October 1857, from which the following particulars are taken.[1]

The gasifier is shown in longitudinal and in transverse vertical section, Figs. 5 and 6, "and consists of a furnace, very like a locomotive firebox in shape, having on all sides, except beneath

FIG. 5. FIG. 6.



BEAUFUMÉ PRODUCER.

the grate-bars, a water-space or water-jacket, which prevents loss of heat by radiation, and in fact constitutes a small detached boiler raising a certain amount of steam." . . . . "The furnace is filled with fuel to a considerable height, say 20 to 28 inches, according to the quality of the coal. The coal is introduced by two double-valved passages, in the roof of the furnace. When the outer cover of either passage is opened, the valve at the inner end is closed to prevent the escape of gas. When the fuel is introduced into the passage, the outer valve is closed, and the inner one opened, and the fuel falls into the furnace, no more gas escaping from the furnace than the small quantity filling the passage. The proper quantity of air required for combustion in the producer is supplied by a blowing-fan which forces it through the firebars." "The air enters the ash-pit by apertures on opposite sides," and the carbonic oxide and other gases pass off by an

[1] Transactions of the Institution of Engineers in Scotland, vol. i. p. 14; and Mémoires de la Société des Ingénieurs Civils, 1866, p. 585.

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outlet near the top of the producer, and are conveyed by a pipe to the furnace. The air-supply for the combustion of the producer-gases in the furnace is delivered by the same fan, and is made to traverse a circuitous passage which is heated by the waste gases from the furnace. The producer-gases issue into the furnace from the pipe by a series of passages arranged in a horizontal line at short intervals apart, while the air for their combustion issues by alternate passages. By this arrangement the air and gases become well mixed before combustion is completed.

Recent inquiry as to the fate of this plan, made by friends of the Author, has elicited from the French naval authorities the information that the Beaufumé apparatus was given up because of the inconvenience entailed by the use of two boilers. This is not very clear, but presumably the water-jacket of the producer itself is reckoned as one boiler, and if supposed to be an ordinary steam-generator, it is of course easy to understand the objection.

Siemens.—From statements made by Mr. Turck to the French Society of Civil Engineers, it appears that Mr. F. Siemens began about this time to patent his ideas connected with regenerative-furnaces, and these plans were introduced into Great Britain by his brother, the late Sir William Siemens, who followed in the year 1861[1] with a patent for a gas-producer having a sloping grate and bars. This is the well-known Siemens producer, Fig. 7. The grate is evidently formed on the model of the "step" grate which is used on the Continent, or on that of the "Chanter" grate, and this feature, with the introduction of a wrought-iron cooling-tube, constitutes the main novelty in the design of this producer.

The cooling-tube (having not less than 60 square feet of surface per gas-producer) generally commences at the top of a brick shaft or up-take, 8 to 10 feet high, placed above the producer at the back, and its object is to cool the gases issuing from the producer, thus giving them increased density, causing an onward movement or current towards the furnace, and rendering it unnecessary to place the producer at a much lower level than that of the furnace. This cooling, however, led to many annoyances through the condensation of tar which resulted from it, and modifications have been proposed in the producer and its working, which

[1] In the Paper by Mr. Turck, entitled "Notice sur les appareils Fumivores, &c.," published in the Mémoires de la Société des Ingénieurs Civils (1866, p. 585), February 1857 is given as the date of the French patent for the Siemens producer.

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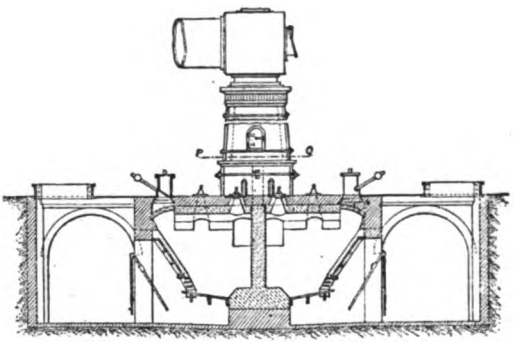
were designed to afford an escape from such difficulties. Sir William (then Mr. C. W.) Siemens proposed to close the ash-pit by a door, and to force air in under the bars by means of an improved form of steam injector; but the disposition of the grate in this producer, with its comparatively thin layer of fuel, prevents the success of a rapid combustion such as would ensue from that plan of forcing. Such a plan answers well with furnaces, where the coal is entirely burned to carbonic acid, or with producers having a considerable column of fuel above the zone of combustion, but a comparatively slow rate of combustion is essential to the success of this form of producer. The Siemens producer is therefore almost invariably worked by means of a slow draught caused by the cooling of the gases.

FIG. 7.



SIEMENS PRODUCER.

Regarding this cooling, Mr. Siemens has said: "The gas rising from the producer at a temperature of about 1,100° Fahrenheit, is cooled as it passes along the overhead tube, and the descending column is consequently denser and heavier than the ascending column of the same length, and continually overbalances it. The system forms, in fact, a siphon in which the two limbs are of equal length, but the one is filled with a heavier fluid than the other. The height of cooling-tube required to produce as great a pressure in the flue as would be obtained by placing the gas-producers say 10 feet deeper in the ground may be readily calculated. The temperature of the gas as it rises from the producers has been taken as 1,100° Fahrenheit, and we may assume that it is cooled in the overhead tube to 100° Fahrenheit, an extent of cooling very easily attained. The calculated specific gravity,

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referred to hydrogen, of the gas (of analysis No. 7) being 13·14, we obtain the following data:—

Weight of the gas per cube foot at		1,100° F.	= 0·022 lb.
,,	,,	,,	100° F. = 0·061 ,,
,,	atmospheric air	,,	60° F. = 0·076 ,,

and from these we have, on the one hand, the increase of pressure per foot of height, in a flue rising directly from the gas-producer

$$= 0{\cdot}076 - 0{\cdot}022 = 0{\cdot}054 \text{ lb. per square foot;}$$

and on the other hand, the excess of pressure at the foot of the downtake from the cooling-tube, over that at the same level in the flue, leading up from the gas-producer (for each foot in height of the cooling-tube)

$$= 0{\cdot}061 - 0{\cdot}022 = 0{\cdot}039 \text{ lb. per square foot.}$$

The height of the cooling-tube above the level of the flue that will be sufficient to produce the required pressure, equal to 10 feet of heated gas column, is therefore,

$$\frac{0{\cdot}054}{0{\cdot}039} \text{ 10 feet} = \text{13 feet 10 inches, or say 14 feet."}^{1}$$

The amount of heat lost by this cooling is not very great, "for as 100 volumes of the gas require for combustion about 130 volumes of air, including 20 per cent. above that theoretically required, the heat given off in cooling the gas 1,000° is no more than would be lost in discharging the products of the complete combustion of the fuel, at a temperature 435° in excess of the actual temperature of 200°, and this loss is greatly diminished if a richer gas is obtained."[1]

Where large quantities of fuel have to be gasified, this system necessitates the construction of plant of extended size and considerable first cost, and this generator does not afford much advantage over those previously introduced. In fact, it can scarcely be gainsaid that it is to the undoubted excellence of the Messrs. Siemens' system of regenerative-furnaces that this producer owes its having occupied a prominent place as a generator of gas, and having either displaced those forms which were introduced at an earlier date, or held its own so long against improved forms which were subsequently proposed.

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[1] The Journal of the Chemical Society of London, vol. xxi. 1868, p. 293.

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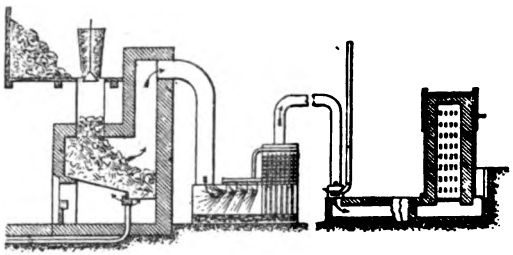
Lundin.—An important addition to the Siemens producer, which is, however, applicable also to the gases from other producers, was introduced by Mr. F. Lundin, of Carlstad-Munkfors, in Sweden,[1] about the year 1863, and consisted of chambers for cooling and washing the gas by means of sprays of water, and then allowing it to pass through tubes or other surfaces over which water was caused to trickle (Fig. 8). This was done primarily to remove all traces of steam from the gas, so that moist fuel, such as sawdust or peat, might be used in the producer for the generation of gas, which could be employed for producing high temperatures. At the same time the cooling and washing undoubtedly removed other substances, besides completing the purification of the gas from tarry hydro-carbons, and it constitutes Mr. Lundin the leader in the modern practice of washing producer-gases.

FIG. 8.



LUNDIN GAS-WASHING APPARATUS.

Bérard.—Mr. Bérard also carried out about this time a process of washing producer-gases, as applied to the gas from a close producer of his own design, his object having been chiefly to remove sulphurous compounds and other impurities from the gas. His producer was[2] "a kind of cupola furnace, the principal feature of which was a movable bottom for the removal of clinkers." Mr. Bérard proposed elaborate contrivances for the purpose of preventing trouble from the clinkering of the ash and small breeze, which is always found a difficulty in producers worked with air-blast alone.

Benson.—A plan of considerable promise was introduced by Mr. G. Henry Benson in 1868, and successfully worked under his direction for some months at ironworks near Cleckheaton (Figs. 9, 10,

[1] F. Kohn, "Iron and Steel Manufacture," p. 142. Cleveland Institution of Engineers. Proceedings. Feb. 9th, 1871, p. 30.

[2] The Iron and Steel Institute. Transactions. Vol. i. 1869, p. 196 *et seq.*

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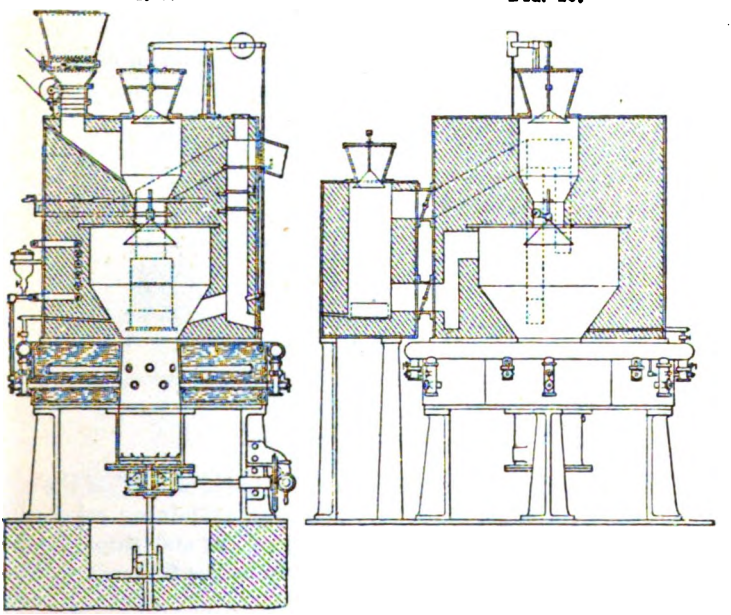
11, 12). It does not seem to have been introduced elsewhere, but was described by him in a Paper to the Iron and Steel Institute in 1869.[1] Fig. 9 illustrates the producer as used for free-burning or non-caking coal, coke, and peat. It is a close vessel, formed in part of a casing of wrought-iron plates, with a lining of refractory material, and resting upon cast-iron standards about 6 feet from the ground. Fuel is charged either by a hopper and cone or by mechanical feeding arrangements, with rollers for breaking up large pieces, and the air required to support combustion is supplied

FIG. 9. FIG. 10.



BENSON PRODUCER.

from a fan or blower, and is led from the main pipe by separate connections to the tuyeres, which are placed in two rows at equal distances round the lower portion of the producer. Hot blast is used, and steam, superheated by a coil in the brickwork lining, is also introduced at various points in the circumference of the same narrow portion near the bottom. This part of the producer is formed of cast-iron boxes, which are filled with water, a method of construction adopted in order to avoid the difficulty caused

[1] The Iron and Steel Institute. Transactions. Vol. i. 1869, p. 196 *et seq.*

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by ash and clinker adhering to brick sides when these were used in the producer at this point. The bottom of the producer is formed of a movable iron plate carried on a vertical iron support, by means of which it can be raised, lowered, or revolved. It has a number of spikes on its upper surface, in order to break up clinker and to agitate the mass resting on it. This plate is gradually lowered as the ash accumulates, and when at its lowest point the ash is removed through a door. Openings are provided in the walls of the cast-iron box under the water-boshes, for inserting bars to support the fuel in the producer while the ash is being cleaned out. Passages and valves are provided, by means of which the hydrocarbon gases containing tarry matter can be made to pass through the hot carbon in the producer, in order to decompose the tarry hydrocarbons. In one arrangement, Fig. 10,

FIG. 11.

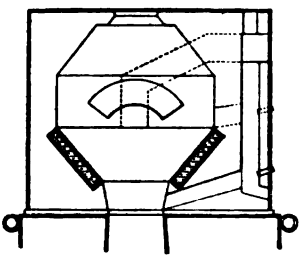
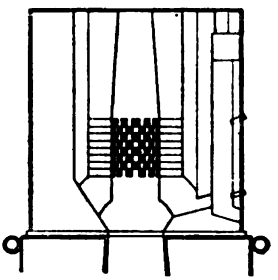


FIG. 12.



BENSON PRODUCER.

Mr. Benson proposes to accomplish this "purification" of the gases by the use of a small subsidiary chamber, containing coke, which is to be kept at a red heat by a blast of air, and through which the gases are made to pass on their way to the furnaces in which they are to be consumed.

Minary.—Mr. E. Minary, of Besançon, proposed in 1868[1] two forms of gas-producer (Figs. 13, 14, 15, 16, 17, 18), one form having a flat grate with bars and a closed ashpit, and the other a closed brick bottom in section like a gothic arch inverted, and at the apex of the curves a tapping-hole by which ash and cinder were run out when melted. Air-blast was delivered under the bars in the closed ash-pit in one case, and from passages opening at an angle downwards from pipes running along two sides of the producer, and imbedded in the brickwork near the bottom, in the other

[1] "Publication Industrielle des Machines, Outils et Appareils," vol. xviii. p. 195, plate 18.

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Fig. 13.

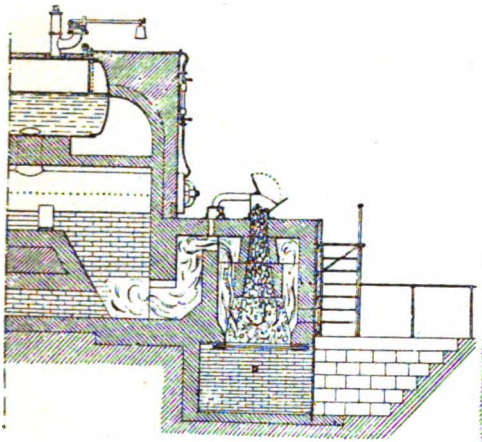


Fig. 14.

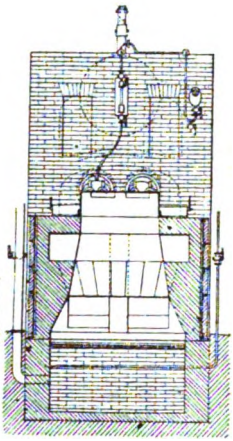


Fig. 15.

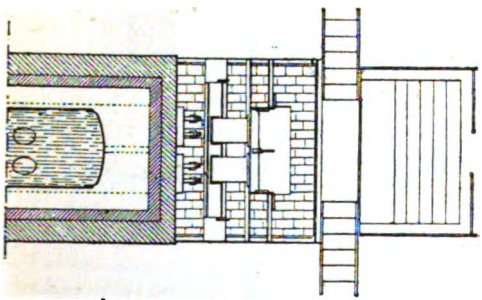
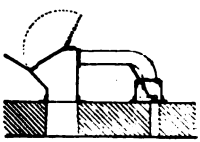


Fig. 16.



Minary Producer with Grate-Bars.

Fig. 17.

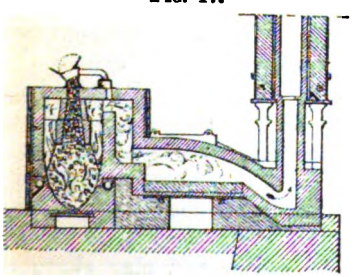
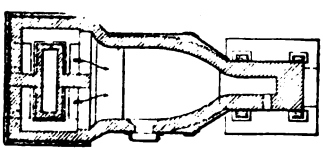


Fig. 18.



Minary Producer with Closed Bottom.

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case. Air was also led in both cases into the coal-hopper, and down through the fuel. In the producer with grate-bars steam was likewise admitted under the bars.

Both producers were on sectional plan of rectangular form, like a flat-sided parallelogram, there being an inner rectangle tapering downwards from the charging-hopper for about half the height of the producer, carried by arches, and surrounded by a chamber or space into which the hot gases escaped from the fuel, the object of this construction having been to heat the coal from the outside of this tapered portion. In order to fuse the ash in the closed bottom producer, limestone was mixed with the fuel in proportions varying according to the amount of ash in the fuel.

FIG. 19.

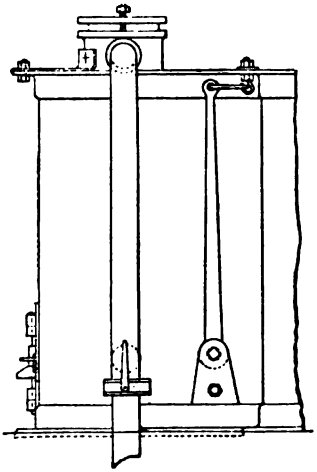
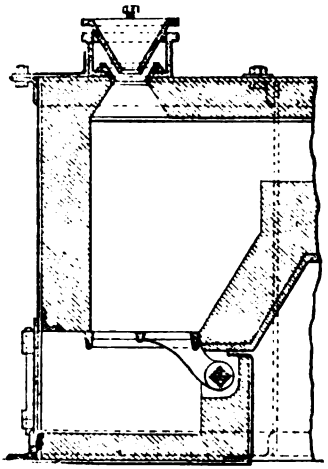


FIG. 20.



WITTENSTRÖM PRODUCER.

Wittenström.—The chamber for forced combustion, which Mr. Wittenström applied to furnaces, was really a gas-producer, although not worked to realize the best advantage from such an appliance. It was introduced into this country in 1871, having been then patented in connection with what was called the "Lancefield furnace," from its being first erected at the Lancefield forge, in Glasgow. Mr. Wittenström had, however, established several of these furnaces previously in Sweden. The combustion chamber (Figs. 19, 20) was merely a deep, square chamber of firebrick, cased in iron, and having a hinged grate with a closed ash-pit. Blast was introduced into the ashpit and also above the fuel from a channel surrounding the charging-hopper, which

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was formed like the nozzle of an injector (Fig. 20), and the gases in a state of ignition were led off by a passage, near the top of this chamber. It was the intention of the inventor that after the accumulation of a few inches of fuel on the grate-bars, the fuel should be fed in by hand in small quantities at regular and short intervals. The uncertainty and irregularity of this method of feeding went very far to prevent permanent success with this apparatus, although some excellent results were obtained with it. A modification of the arrangement, with mechanical feeding appliances added, was patented by the Author in 1876 for application to steam-boilers, but has not been carried out in practice.[1]

Kidd.—In 1875 Mr. Joshua Kidd introduced a new form of gas-generator[2] (Figs. 21, 22, 23), the purpose of which was to produce a modified water-gas, principally for domestic heating and cooking.

FIG. 21. FIG. 22.

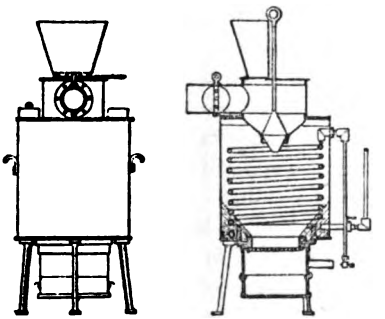
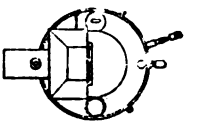


FIG. 23.



KIDD PRODUCER.

It consisted of a cylindrical chamber of iron standing vertically on three cast-iron supports, which enclosed a cast-iron cylindrical ashpit, jointed in the middle so that the lower half could be detached for the removal of ashes. The only opening to the ash-pit during work was a pipe into which a jet of steam was blown, inducing a current of air along with it. This current of steam and air entered under movable grate-bars upon which the fuel rested. With the exception of a small tapered ring of firebrick lining at the bottom of the chamber just above the grate-bars, the iron chamber was not lined, but had near its sides a coil of iron pipe for generating the steam required in working the apparatus. The

[1] Minutes of Proceedings Inst. C.E. vol. liv. plate 6, fig. 1.
[2] Engineering, 1875, vol. xix. p. 326.

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fuel was fed in through a hopper with a conical plug placed in the top of the chamber, from which hopper a chimney branched off having a damper. This chimney was for use in lighting up the generator until steam was produced. The gas-exit was an opening in the flat part of the top of the generator.[1]

Lowe and Strong.—Some ingenious apparatus for producing a high quality of water-gas was introduced in America about the year 1874. There are two varieties of this apparatus, designed respectively for making illuminating-gas and heating-gas, and these are known as the Lowe and the Strong apparatus. They have been extensively adopted in America, besides having been introduced in England, Sweden, and Prussia with varying success.

Much interesting information on the subject of these processes is to be found in a pamphlet issued by the Strong Gas, Fuel, and Light Co., of New York, and in notices in "Engineering" of the 14th of November, 1879, the 3rd of April and the 30th of October, 1885, while references to the English and German experiments are to be found in The Journal of the Iron and Steel Institute.

The Lowe apparatus (Fig. 24) consists of a generator, a superheater, a washer or purifier, and a scrubber, the generator and superheater being the essential and characteristic portions of the apparatus. These are in form like cupola furnaces, built of brick inside a casing of wrought-iron, the superheater being about half as high again as the generator, and having a chimney or smoke-stack rising from its top. Near the bottom it has an air-space or combustion chamber formed by an arch of brickwork perforated with numerous holes, and above the arch the superheater is nearly filled with loosely-stacked fire-bricks. A pipe connects the bottom of the superheater with the top of the generator, and another pipe, controlled by a valve, is provided to lead the gas from the top of the superheater to the purifier or the hydraulic-main. An opening at the bottom is also provided for the admission of an air-blast; and a valve in the chimney gives the means of shutting it off at will. The generator has a charging-door and valve at the top, underneath which the ends of some small pipes are inserted for the purpose of dropping oil upon the charge of fuel. At the bottom of the generator are grate-bars, and a closed ashpit into which an air-blast is delivered. A pipe is provided for conveying steam into the hot fuel when necessary. In working this apparatus, the generator is charged with anthracite, broken to a large egg size,

[1] "A New Method for Producing Cheap Heating Gas, etc." By S. W. Davies. Journal of the Society of Arts, 1878, vol. xxvi. p. 444.

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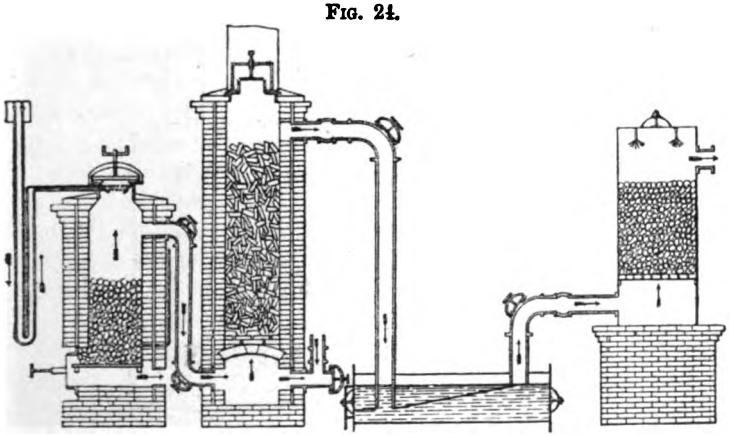
through the opening in the top, which is then closed, and the valve to the chimney being open while the gas-outlet is closed, the coal is fired with the aid of an air-blast under the grate. The gases formed by combustion, which are principally carbonic oxide and nitrogen, are driven off through the pipe above to the chamber at the base of the superheater, where, meeting an air-blast, they ignite and flame up through the mass of loose brickwork, which they gradually bring up to a white heat, while the body of coal in the generator becomes cherry-red. This is the proper condition for gas-making. The valve is then closed, and the air-blast shut off, the gas-outlet is opened, and at the same time steam, preferably superheated, is blown into the mass of coal in the generator. In contact with the incandescent carbon the steam

FIG. 24.



LOWE WATER-GAS PRODUCER.

forms water-gas, consisting almost entirely of hydrogen and carbonic oxide, and at the same time small streams of crude petroleum are dropped directly upon the top of the red-hot coal, and becoming volatilized add hydrocarbon gases to the escaping water-gas. The mixture of gases passes to the bottom of the superheater, and thence upwards through the mass of white-hot brickwork to the outlet which conducts the gas to the washing-apparatus, or to a hydraulic-main. The action necessarily cools both the coal and the bricks of the superheater, but the superheating action completes the manufacture of the gas, and at the same time arrests any particles of carbon which may be carried over from the generator. As soon as the temperature falls too low for proper admixture, the supplies of steam and hydrocarbon are stopped;

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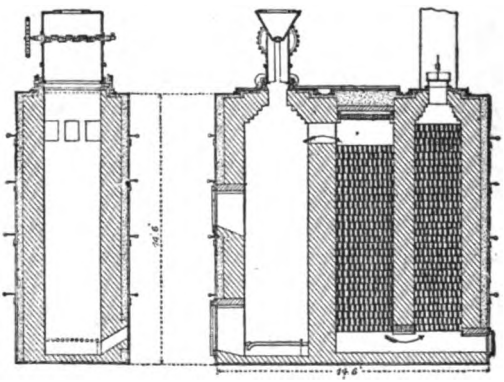
a little more coal is then added to the generator, the blast is again turned on, and the process is recommenced. It is claimed for this process that the superheater, which does not become cooled so rapidly as the fuel in the generator, maintains a uniform and permanent composition of the resulting gas in spite of the changing condition of the generator, and that gases of lower density are produced by its means than by the retort-system in use in gasworks.

The Strong apparatus for producing heating-gas is rather more simple, but also follows the intermittent method of working. The generator (Fig. 25) is cupola-shaped, but has a greater height than that of the Lowe apparatus, and there are two secondary chambers or superheaters filled with bricks, regularly

FIG. 25.



STRONG PRODUCER.

stacked as in a Siemens "regenerator." The generator is charged with coal or coke, and fired by an air-blast admitted beneath the water-grate. The ascending gases pass through an opening near the top into the first superheating chamber, where they are met by a second air-blast admitted through a perforated grating at the top of this chamber. The gases being ignited by this means burn down through the brickwork of the first, and upwards through that of the second, superheating chamber, escaping by the valve and the chimney. By the time the coal in the generator is red, the bricks of the superheaters are nearly white-hot. The air-blast is then shut off, the valve leading to the chimney is closed, and steam is admitted just underneath it. This steam becomes very highly heated, and is partially, if

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not wholly, decomposed in contact with the hot bricks. As it passes in the reverse direction to that taken by the gases as just described, and as it enters the generator, it is met by a shower of finely-ground coal dust, which is fed from a hopper by an archimedean screw. This coal is instantly distilled on coming into contact with the highly-heated gases from the steam, and the resulting combination of gases is passed downwards

FIG. 26. FIG. 27. FIG. 28.

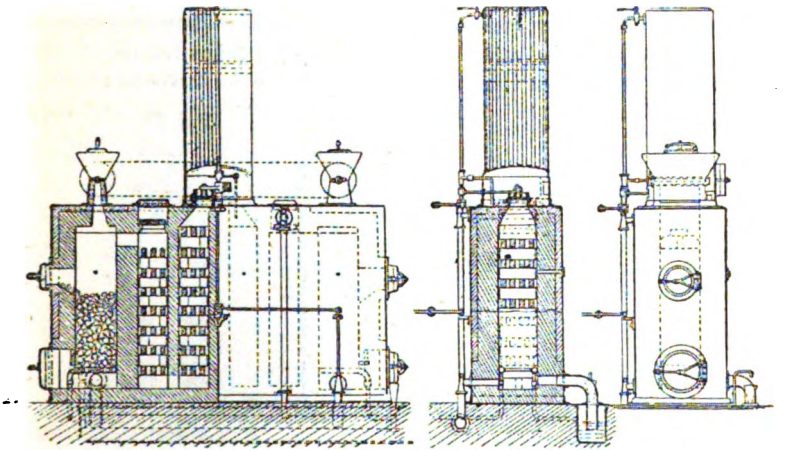
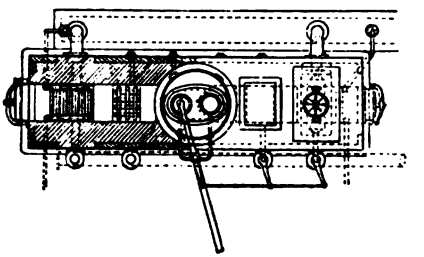


FIG. 29.



STRONG WATER-GAS APPARATUS. WITTENSTRÖM'S IMPROVEMENTS.

through the red-hot fuel, which reduces the carbonic acid contained in them, and removes any undecomposed carbon, and thence by an opening under the grate to a hydraulic-main.

Several plans have been proposed for rendering this class of apparatus more nearly continuous in working, one of the best of which was designed by Mr. C. Wittenström during the trials of the Strong apparatus in Stockholm in 1879. In this plan

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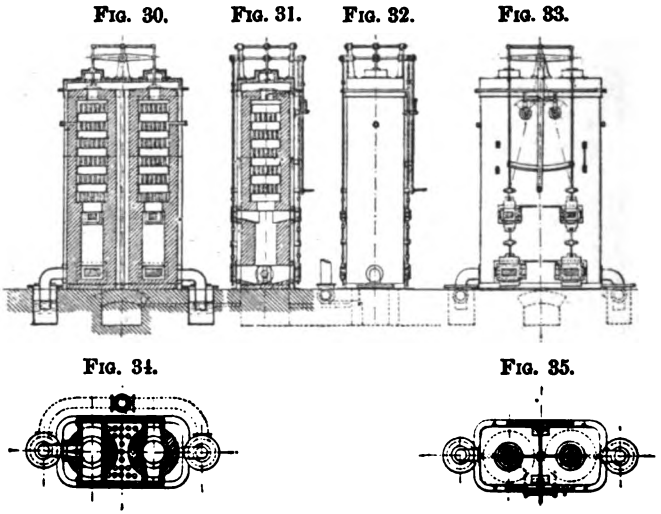
(Figs. 26, 27, 28, 29) there was a duplicate set of apparatus on each side of a central chimney, which was surrounded by a boiler for generating steam. The various steam, air, and coal-dust valves for each set were combined together by means of levers, which were all coupled to a main lever operating the chimney-damper, so that by one movement each set of valves was simultaneously opened or closed along with either passage opening to the chimney. In another modification (Figs. 30, 31, 32, 33, 34, 35), Mr. Wittenström proposed to mount the superheaters vertically above the generators, placing a pair of combined chambers side by side, with a space between them, filled up by tubes for generating steam, the gases from the superheater passing downwards through



Strong Water-Gas Apparatus. Wittenström's Improvements.

these tubes on their way to a chimney-flue placed underneath the whole apparatus. This arrangement permitted of a very simple method of working the valves by levers. More recently, Mr. W. A. Goodyear[1] has proposed a plan with one generator common to two sets of superheaters,[2] and another plan has been patented by the Europeiska Wattengas Actiebolaget of Stockholm,[3] while Mr. W. Crossley has patented a fresh arrangement.[4]

[1] Transactions of the American Institute of Mining Engineers, vol. xi. 1883, p. 301.
[2] The Journal of the Iron and Steel Institute. 1883, p. 343.
[3] The Journal of the Society of Chemical Industry. 1883, p. 370.
[4] British patent, No. 589. 1883.

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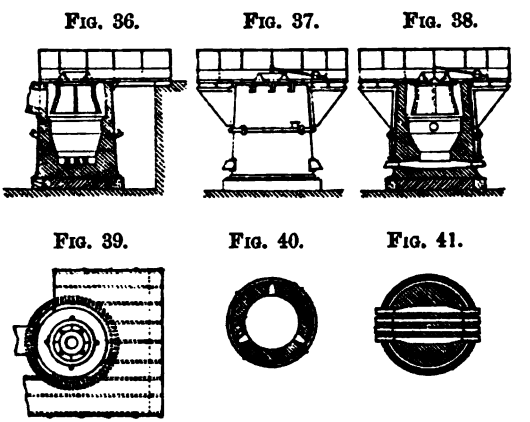
Tessié du Motay.—The gas-producer with which the name of Mr. Tessié du Motay, a well-known French metallurgist, is associated, was first brought prominently before the notice of engineers in this country and in America by the late Mr. A. L. Holley, LL.D.,[1] although it appears that it was known in France in 1871,[2] and that some results of its working in Paris and at Terrenoire were communicated to the French Society of Civil Engineers. In a communication to that body in 1874, Mr. Lencauchez, however, disputes the accuracy of the results reported by Messrs. Tessié du Motay and Pourcel.

The producer, as described by Holley (Figs. 36, 37, 38, 39,



Tessié du Motay's Producer.

40, 41), was a cupola-shaped furnace, with closed hearth, built inside of a wrought-iron cylindrical casing, the sides having different diameters at various portions of their height, and having one portion built with an incline like that of the boshes of a blast-furnace. Underneath the cup and cone charging-hopper, a bell-shaped cylinder of wrought-iron was hung to direct the charge of fuel, and leave an annular space at the top of the producer, with which the branch-pipe was in communication. The producer was worked by an air-blast, a belly-pipe encircling it at about half its height, and having branches leading down to four cast-iron boxes placed on two opposite sides, and form-

[1] Transactions of the American Institute of Mining Engineers, vol. viii. 1880, p. 27. Engineering. 1879, vol. xxviii. p. 457. Minutes of Proceedings Inst. C.E., vol. lxxiii.

[2] Mémoires de la Société des Ingenieurs Civils. 1874, p. 883.

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ing the terminations of four grooves or channels, running across the hearth, for the accumulation of ash and scoria. By opening the covers of the two boxes at the ends of any groove, the refuse material could be pushed through from one side, and discharged at the other. Any clinker adhering to the brickwork of the sides of the producer could not be removed by the above method, and this producer is open to the objection common to all those worked by air-blast—that of necessarily working too hot, and giving rise to a comparatively large proportion of carbonic acid in the gas. It seems from Holley's remarks that there was some loss from coke being mixed with the ash, &c., in cleaning. Practically the same plan, but with the addition of a steam-pipe led into the blast-boxes, was described by Mr. D. Cowan to The Mining Institute of Scotland,[1] as being in process of erection in March 1884 at the Carron Ironworks.

Wilson.—What is now known as the "Wilson gas-producer" was patented by Messrs. Brook and Wilson in 1876, but was introduced by Messrs. Howson and Wilson in connection with the Godfrey and Howson puddling-machine at Middlesbrough.[2] At first, it was made rectangular on plan, and in vertical section had some resemblance to one of Mr. Minary's[3] designs, which resemblance doubtless caused Mr. F. Lürmann[4] hastily to assume that the plans were identical. The resemblance is, however, limited to the possession by both designs of a tapered upper portion of the interior, into which portion the fuel is fed, and which is in Minary's producer partly surrounded by a passage or chamber for the exit of the gases. In the Wilson producer this portion is entirely surrounded by the chamber for hot gases.

Both inventors had the same idea in view in this construction, in as far as the intention was to transfer heat to the freshly-charged coal from the hot escaping gas. In the Wilson producer it was further intended, that this upper portion should be a veritable retort, where the preliminary distillation of the fuel should take place, the gases from which should have to pass through hotter fuel on their way to the exit passages, so that they should thus be freed from the heavier hydrocarbons, which readily deposit as tar, in the gas-mains. In the same producer a novel method of supplying the

[1] Transactions, vol. v. p. 335.

[2] Papers by R. Howson in the Journal of the Iron and Steel Institute. 1877. Engineering. 1877, vol. xxiv. p. 255.

[3] Cleveland Institution of Engineers. Proceedings. 1878–79, p. 221.

[4] The Journal of the Iron and Steel Institute. 1880. Engineering. 1880, vol. xxx. p. 200.

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blast was applied to a producer with a closed hearth without grate-bars. This is a steam-jet, on the "injector" principle, entering the narrow throat of a tapered vertical pipe, and inducing a current of air in proportion to the amount and velocity of the jet of steam. In practice it is found that, for satisfactory working, the steam and air stand in the proportion of 1 part of steam to 20 parts of air by weight. The mixture of air and steam is by this plan conveyed into the interior of the producer by a raised channel or "tuyere," laid across the centre of the hearth, and pierced at the central point of the producer by three openings on each side of the tuyere. This arrangement has the advantage of delivering the blast under pressure, so that the gas issues from the producer at a pressure of from 1 inch to 3 inches of water, and of delivering the blast at the farthest point from the walls of the producer, so that contact of both the air and the steam with the fuel is ensured. It has been repeatedly found that when the air-blast is delivered at the sides or walls of a producer, a portion of the air is able to pass up along their surfaces without entering into combination with the fuel, and the resulting gas, therefore, contains uncombined oxygen, besides a larger proportion of nitrogen than is necessary. The decomposition of the steam in the Wilson producer abstracts heat at the bottom, thus protecting the iron tuyere and brickwork, while the clinker is rendered brittle, and prevented from adhering in any great degree to the brickwork. The heat which becomes latent by the decomposition is recovered on the combustion of the hydrogen and carbonic oxide which results, and this adds to the volume of gases from the coal. This form of producer is periodically cleansed by hand, access to the interior being obtained through doors opening on to the hearth at the front of the producer.

Very soon after its introduction the rectangular form was discarded in favour of a circular one (Fig. 42), which permits of the whole producer being cased in wrought-iron, and adds to its compactness and durability.[1] In this form it has been largely used, on account both of its compactness and of its speed of working. The latter is usually at the rate of about 25·7 lbs. of fuel per square foot of hearth-area per hour, as compared with 12·5 lbs. per square foot of grate-area per hour, which is the usual rate for Siemens' sloping-grate producers.

[1] Transactions of the Institution of Engineers and Shipbuilders in Scotland. 1881, vol. xxiv. p. 177. The Journal of the Society of Chemical Industry, vol. i. p. 53. The Mining Institute of Scotland, Transactions, vol. iii. p. 194. Minutes of Proceedings Inst. C.E., vol. lxxiii. plate 15.

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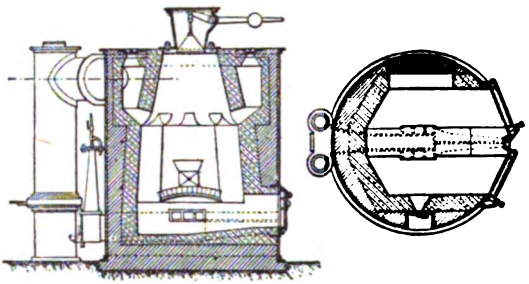
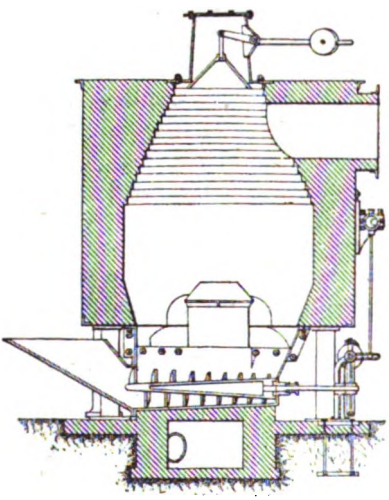
Since the introduction of these producers into America, several plans have been proposed there which contain the main features of the Wilson producer, as applied generally to one with grate-bars, such as the Philips' and others; while in the Langdon pro-

FIG. 42.



WILSON CIRCULAR PRODUCER.

FIG. 43.



WILSON AUTOMATIC PRODUCER.

ducer, which has a closed hearth, the air and steam are admitted from the steam-jet through openings in the circumference of the lining, leading from an annular passage which communicates with the steam-jet,[1] this being found suitable for working with anthracite.

[1] The Journal of the Iron and Steel Institute, vol. ii. 1883. Transactions of the American Institute of Mining Engineers, 1883, p. 93.

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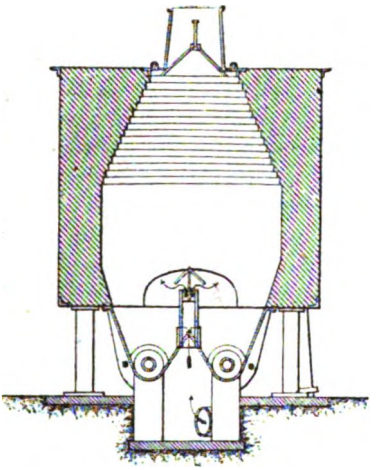
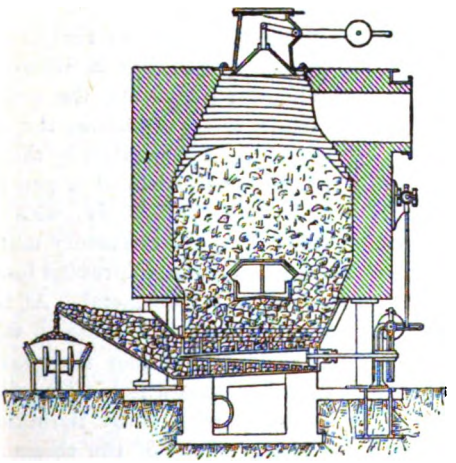
In a later patent (No. 4697, 1882), Mr. Wilson has introduced[1] a new form of producer (Figs. 43, 44, 45), which is the first, and was

FIG. 44.



WILSON AUTOMATIC PRODUCER.

FIG. 45.



WILSON AUTOMATIC PRODUCER.

for a time the only one, providing for the automatic continuous

[1] The Journal of the Society of Chemical Industry, vol. ii. p. 453. Cleveland Institution of Engineers, Proceedings, 1883–84, p. 172.

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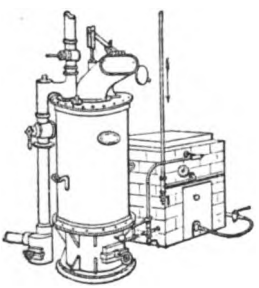
removal of the ash and clinker by machinery. The same means are also applied to the continuous feeding in of the fuel from a hopper above, and if desirable, motion may be given to the ironwork forming the entire bottom of the producer, in order to agitate and break up the mass of fuel, and thus keep it of regular and even consistency. By these means the necessity for manual labour for charging, poking the fuel, or cleaning out, is entirely obviated. The discharging-mechanism, consisting preferably of iron worm-screws, works in troughs containing water, which forms a seal to that part of the producer, preventing the escape of gas, even though a pressure of 6 inches of water is kept up. This water also is a recuperator of heat, as the ash and clinker in passing through it impart their heat, which would otherwise be uselessly dissipated outside, and they issue by the shoot comparatively cool, while the water evaporated by their heat ascends as steam in the producer, and adds by its decomposition to the heating-gas. Some of the same features are found in a subsequent patent of Siemens (No. 663, 1883), and in one of L. Mond (No. 3821, 1883).

Dowson.—A compact arrangement has been effected by Mr. J. Emerson Dowson, M. Inst. C.E. (Patent No. 3,997, 1878, and No. 2,859, 1881), for supplying gas for the smaller industries and for domestic purposes. It has been also successfully applied to the production of gas for working "Otto" gas-engines.[1] This producer (Figs. 46, 47, 48) follows more in the line of that of Kidd than in that of Wilson, but it differs from Kidd's generator in having the coil of pipe for raising or superheating the steam-supply separate. As described by the inventor, the apparatus consists of a generator with a cylindrical iron body, which encloses a thick lining of refractory material, as in a foundry cupola, to prevent loss of heat and oxidation of the metal. At the bottom of this cylinder is a grate on which the fire is built up. Under the grate is a closed chamber, into which a jet of steam plays, carrying with it by induction a current of air, and by reason of the pressure of the steam the mixture of steam and air is forced upwards through the fire. The

FIG. 46.



DOWSON PRODUCER.

[1] Prof. W. E. Ayrton "On the Economical Use of Gas-Engines," 1881. Engineering, 1882, vol. xxxiii. p. 469; 1883, vol. xxxv. p. 130. Minutes of Proceedings Inst. C.E. vol. lxxiii. p. 311.

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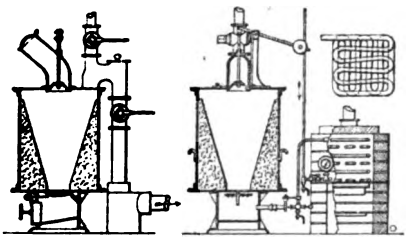
steam is superheated before it enters the generator, and should have a pressure of 20 to 25 lbs. per square inch. The steam is produced and superheated in a zigzag coil of pipe enclosed in a separate superheater or furnace. The coil is heated by some of the gas from the producer or from a gas-holder, except when the apparatus is being started, and then a fire is lighted under the coil. A hopper is provided for charging the producer with fuel, and an exit-pipe, which is also on the top surface of the cylindrical casing, and has a vertical branch, used as a chimney when the producer is being started. The brick or ganister lining of the producer tapers downwards in a straight cone from the full diameter of the casing at the top to the small diameter of the movable grating at the bottom. For many heating operations, ordinary coal may be used in this apparatus, but anthracite is preferred. Purifying and washing-appliances are generally combined with it,

FIG. 47. FIG. 48.



DOWSON PRODUCER.

these being demanded by the purposes to which the gas is usually applied.

Gröbe and Lürmann.—Starting from the principle that a good gas-producer should have two distinct chambers, in which respectively the hydrocarbon gases should be distilled from the coal, and the resulting coke should be converted into gas by means of air, Messrs. Gröbe and Lürmann in 1877 devised their producer to fulfil these requirements.[1] In a communication to the Iron and Steel Institute at their meeting at Düsseldorf in 1880, Mr. Lürmann claimed that "all generators constructed hitherto have only had one chamber in which the gases contained in the fuel are expelled and the resulting coke is converted." "The green damp fuel," he said, "is charged cold and unprepared, from time to time, upon the materials which are in process of decomposition, similar to the process which takes

[1] The Journal of the Iron and Steel Institute, 1882, p. 351.

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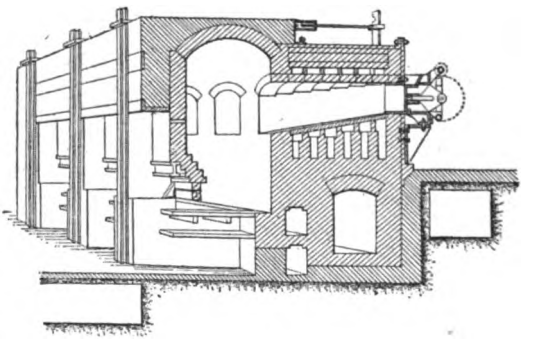
place on an ordinary grate, and as the two processes of expelling and of converting require entirely different conditions, the quality and quantity of the resulting gases are constantly changing. To expel the gases contained in the fuel no atmospheric air is required, but only heat, whilst for the conversion (combustion) of the remaining solid parts (coke) into carbonic oxide, atmospheric air is essential. It requires no heat, but, on the contrary, throws heat off, which is partly employed for expelling the gases from the fuel; and, having thus become latent, it cannot be utilized in the combustion chamber. With all firing arrangements in which high temperature is the object, it is of importance not to lose heat by radiation or conduction, nor to allow any of the heat to become latent."[1] These statements, however, need some qualification, because in both the operations of distilling the hydrocarbons from

FIG. 49.



GRÖBE PRODUCER.

coal and of volatilizing the solid carbon by transforming it into carbonic oxide, heat is absorbed, and so far becomes latent. It does not matter much whether that heat is supplied partly by the hot producer-gases, or wholly from other sources, the amount required remains the same. This heat is, in part at any rate, recovered on the combustion of the gases so formed.

Moreover, both the Minary closed-hearth producer and that of Wilson are admitted by Mr. Lürmann to have distinct portions in which the two processes are carried out; but his objection to these producers takes the form that the expulsion-chamber is not quite distinct from the conversion-chamber, but is only an elevated part of it, and that the heat of expulsion is drawn from the heat pro-

[1] The Journal of the Iron and Steel Institute, 1880, p. 578.

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duced by the conversion, or, more correctly, by the combustion of some of the carbon to carbonic acid during its conversion, and thus becomes latent. There is, moreover, no similarity between these forms of gas-producers and an ordinary grate, for in the upper or "retort" portion of the producers the coal is heated from the outside without access of air, and therefore the difference between them and the Gröbe producer is merely one of degree. In the Gröbe generator (Fig. 49) the chambers for the different operations are quite distinct, a horizontal chamber heated from the outside by waste-gases from furnaces, which gases traverse the passages constructed for that purpose, being provided for the distilling operation. One end of these horizontal chambers (there being several of them side by side) opens into a vertical chamber, which extends along the row of horizontal chambers, and is fitted with grate-bars. Into this vertical chamber the red-hot coke from the horizontal chambers is pushed from time to time. Passages are provided for heating the air-supply for combustion in the vertical chamber, and the mixture of hydrocarbon gases and carbonic oxide and nitrogen is withdrawn by a common exit-tube.

This gas-producer is said to have worked with great regularity and economy, and to have displaced the Siemens' generators at the Osnabrück Steel Works.

Sutherland.—Mr. W. S. Sutherland's first patent for welding by means of gas was taken out in 1869, but he did not patent a gas-producer until 1874 (No. 1784). This producer (Figs. 50) was a tall cylindrical chamber, consisting of a metal casing, with a brick lining, having an annular space between the casing and brick, containing sand, in which space vertical tubes were placed to convey the blast of air for combustion down to another space formed by the brick lining of the generator. The brick lining was built parallel at the top of the producer, then slightly inclined towards the centre, just above the point where air was admitted, at which point the lining was widened out considerably in order to form a broad annular opening for an air-space, the lining above this sudden enlargement being held up by an angle-iron ring fastened to the metal casing. Underneath the air-opening the larger diameter of firebrick lining was continued so as to form a chamber for the collection of ashes and refuse, which were periodically removed by a suitable door. A vertical iron poker in the centre of the hearth or bottom was worked by a lever underneath the bottom of the producer, the arrangement being similar to that in the Benson generator.

A charging-hopper was provided at the top of the producer, and

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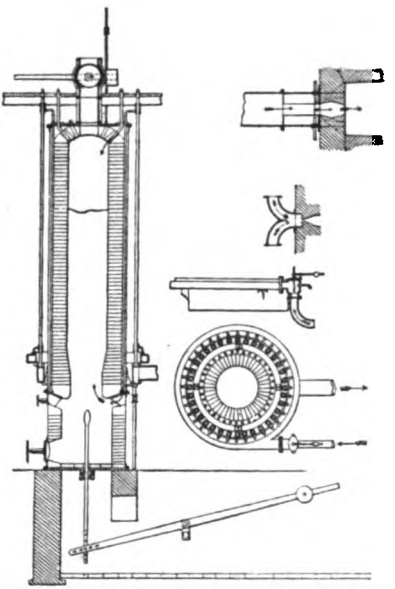
there were numerous small iron pipes issuing radially through the top of the producer to convey the gases. These pipes, as soon as they dipped vertically, were enclosed in larger pipes, which carried the blast from an annular blast-box or belt near the bottom to the top of the producer casing, whence smaller blast-pipes descended through the sand lining as described.

Height was an object in this producer, so that the gases might be partially cooled by heating fresh fuel, and it was intended that they should be further cooled by heating the incoming air-blast by means of the arrangement of pipes just mentioned.

FIGS. 50.



SUTHERLAND PRODUCER.

The object of this cooling was that some of the tar and condensable matters should be removed from the gases before their delivery into the main pipes. This was no doubt suggested by the inconvenient accumulations of tar in the Siemens cooling-tube and gas-mains, and is distinct from any purpose of recovering waste products from the fuel. In fact, ammonia is not named by Mr. Sutherland in this patent. The proportions and dimensions, which are, however, shown in Mr. Sutherland's patent drawings for the pipes and passages provided for conducting the producer-gases, and for the removal of the "surplus tar and water" from these

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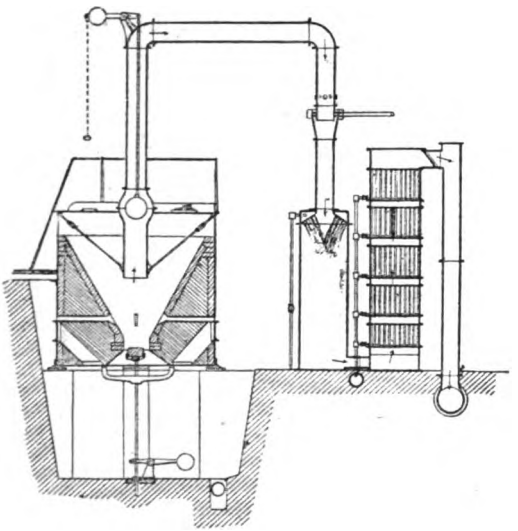
gases, indicate that this arrangement of producer, whatever its merits, could not have been constructed and successfully worked as it is shown.

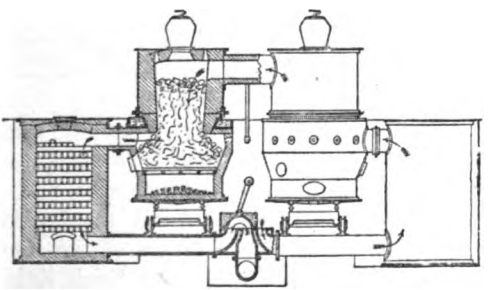
In 1878 (No. 3,274), in 1882 (No. 3,908) (Figs. 51, 52), and in

FIG. 51.



SUTHERLAND PRODUCER.

FIG. 52.



SUTHERLAND PRODUCER.

1884 (No. 8,053), Mr. Sutherland patented improved forms of his gas-producer, these being of a larger diameter compared with their height than the first, and having either a movable plug or valve, or grate-bars at the bottom. Some of these later forms of producer are intended to be worked by having the gases withdrawn by ex-

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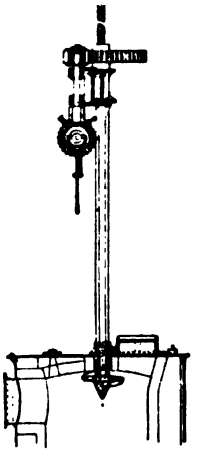
hausting appliances in order to their being passed through condensers and washers. One form (Fig. 52) is shown to be worked by blast, having two producers and two regenerative-chambers working alternately, in order that the gas issuing from a producer should pass through one regenerative-chamber, and should give up part of its heat to it, which would in turn heat the air introduced for combustion—the direction of the currents being reversed by means of a valve.[1] In his latest modification Mr. Sutherland introduces a stirring arrangement (Fig. 53), consisting of a vertical spindle passing down through the top of the producer, and carrying on its end a stirrer to plough up the fuel. This is made preferably diamond-pointed to enter and loosen the fuel, and carries two arms like propeller-blades to screw their way into the mass of fuel for the purpose of breaking it up. The stirrer is driven by overhead gearing, and according to Mr. Sutherland's account,[2] it materially reduces the labour of working the producers.

FIG. 53.



SUTHERLAND PRODUCER.

Siemens.—A circular form of producer was introduced in 1881 by the late Sir William Siemens, probably on account of a widely-expressed partiality for some of the producers of that form. A producer having a strong resemblance to the Siemens new design, was, however, proposed by Mr. B. D. Healey (Fig. 55) in a Paper on "the Manufacture of Steel," which he communicated to the Institution of Engineers and Shipbuilders in Scotland on the 23rd of April, 1872.

The only difference in the Siemens new design (Fig. 54) is that, as first introduced, it was shown with a blast-pipe coiled in the gas-collecting space at the top of the producer, and then carried either up above the producer-top, and then vertically down through the charging hopper to the centre of the mass of fuel in the producer; or first down through the brick lining, and then vertically upwards at the centre of the producer. In both cases the bottom of the producer was open—except that a cone might be placed, apex upwards, at the centre—so that ashes should fall

[1] This is practically the plan of the Siemens regenerative gas and breeze ovens illustrated by Dr. Percy. "Fuel," pp. 456–458.

[2] The Journal of the Iron and Steel Institute, 1884, p. 75.

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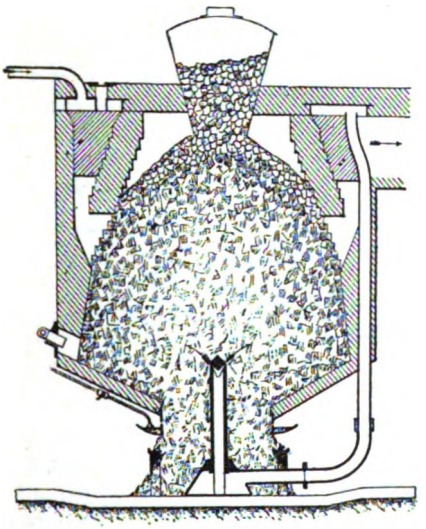
out on to a floor or tray; and water was put in a gutter or tray, so that some vapour or steam should ascend from it into the producer. It was soon seen to be impossible to work a producer having an open bottom such as this with a pressure of blast, and

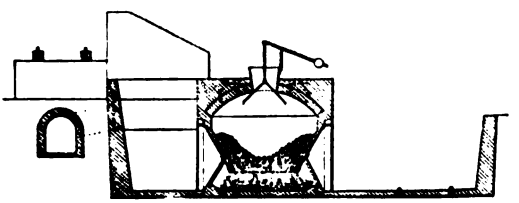
FIG. 54.



SIEMENS NEW DESIGN.

therefore where it has been introduced this producer is worked by chimney-draught, a small steam-pipe being arranged to direct a spray of steam on the hot ashes as they issue from the opening

FIG. 55.



HEALEY PRODUCER.

at the bottom of the producer (Fig. 56). Some steam is thus carried in by the current of air induced by the draught.

To work well, this producer should be placed at a much lower level than that of the furnaces it is to supply, but considering the present requirements of gas-distribution and heating in industrial

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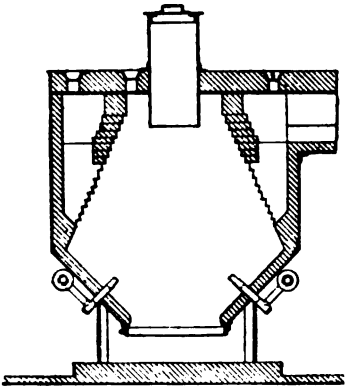
operations, it is rather a step backwards than an advance in the history of the use of such appliances for dealing with fuel.

Fig. 56.



Siemens Circular Producer.

Modified Siemens old-type producer.—Another design, which is a modification of the Siemens old sloping-grate producer, has been more recently proposed by Mr. F. Siemens, and has been described by Mr. Head in a Paper to the Iron and Steel Institute.[1] This modification consists in providing separate exit passages for the hydrocarbon gases distilled from the surface of the fuel, and for the fixed gases produced in the body of the fire. The object of this separation is stated to be that the hydrocarbon gases may be obtained for treatment for the recovery of ammonia and tar, without the necessity of having retorts to produce them, as the late Sir William Siemens proposed. Figs. 57, 58 show this new modification, in "which there is a curtain wall, which comes down to the level of the fuel, or may extend a little below it, dividing the gas-producer into two compartments, so as nearly to separate the zone where carbonic oxide is mostly developed, from the zone where hydrocarbons are given off. When increased action is desired, air may also be admitted to the body of the fuel by one or more nozzles, formed in the brickwork, the supply current to each being maintained by a steam injector. The nozzle and injector are so situated and directed as to provide against blowing flame or gases down through the grating." The hydrocarbon gases which collect in the chamber are led away by a pipe,

[1] The Journal of the Iron and Steel Institute, 1885, p. 126.

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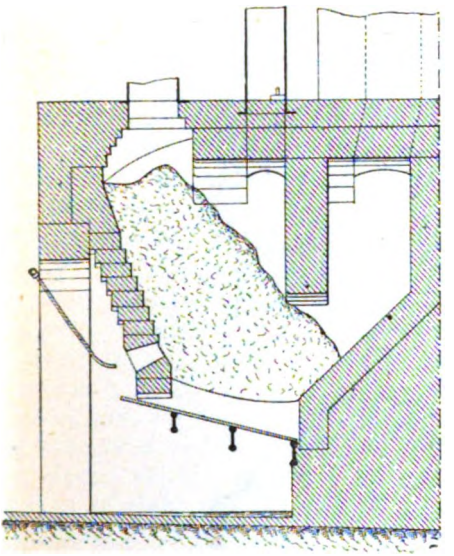
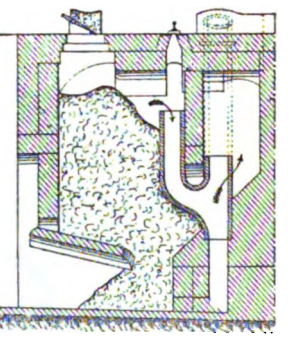
by means of an exhauster, to be treated in the usual manner for the extraction of tar and ammonia. This plan is open to the objection that the action of the cooling-tube, upon which the working of these producers mainly depends, must be seriously interfered with both by the exhauster, which sets up a current in another direction, and by the removal of the condensable gases, which will prevent that portion of the gases which may pass through the cooling-tube from being reduced in bulk or increased in density to any great extent. Moreover the arrangement is proposed for a system of treatment of coal for the recovery of ammonia,

FIG. 57.



MODIFIED SIEMENS OLD-TYPE PRODUCER.

FIG. 58.



MODIFIED SIEMENS OLD-TYPE PRODUCER.

which is at variance with Mr. Foster's published results in connection with that subject.[1] It is all the more remarkable to find, therefore, that in the discussion on Mr. Head's Paper, Mr. Foster stated[2] that "from a scientific point of view Sir William Siemens' apparatus would lend itself to the utilization of the nitrogen of the coal better probably than any apparatus with which he was acquainted." This is surely a mistake.

Other modifications have also been illustrated by Mr. Head in

[1] "The Composition of Coal." Minutes of Proceedings Inst. C.E. vol. lxxvii. p. 162.
[2] The Journal of the Iron and Steel Institute, 1885, p. 155.

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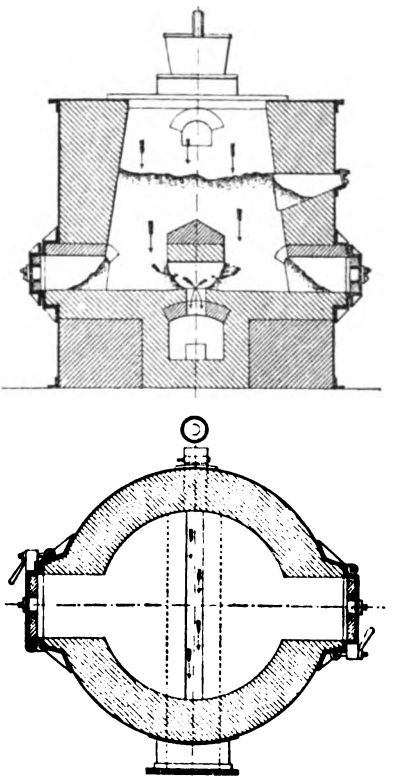
the same communication, the purpose of which is to provide a passage of cast-iron or of brick, in which the tarry vapours may be decomposed by the heat of combustion conducted through the sides of the passage. Probably some decomposition will take place, and in that case a deposit of carbonaceous matter in the passage may be expected.

Howson.—An interesting modification of the closed-hearth pro-

FIGS. 59, 60.



HOWSON PRODUCER.

ducer has been recently proposed by Mr. R. Howson, of Middlesbrough. The novelty in this design (Figs. 59, 60) consists in forcing in the air by a steam-jet above the surface of the charge, or just under the charging-hopper, and leading away the gases from the bottom of the producer, either directly by openings into a central flue, or first into circular channels outside the combustion-chamber,

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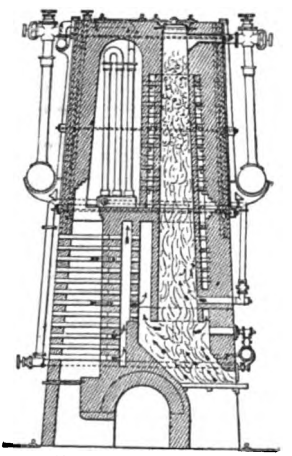
and then into a flue below. A central perforated bridge is sometimes used in order to protect the central slit or openings to the gas-flue below. This producer is easily cleaned, and the effect of passing the gases downwards is not only to break up condensable hydrocarbons, but also to prevent the formation of clinker. The introduction of the steam and air-blast above renders any overheating of the bell-cone impossible, and facilitates the operation of stirring the fuel. It is of course impossible to recover either ammonia or tar from the gases of this producer, and it may be doubted whether--as combustion must proceed upwards against the blast to reach the fresh coal—it will make gas as rapidly as others of the same size. It might be expected also that the greatest production of ash would take place above, where the most intense combustion must necessarily occur.

Young and Beilby.—Although the apparatus, patented by Messrs. William Young and George Beilby, was devised primarily to recover as ammonia the nitrogen in shale and coal, yet as part of it is applicable to the production of heating-gas, and as even in the process of recovery of ammonia from coal some heating-gas of a certain quality is produced, this generator demands a notice among the various methods of producing fuel-gas.

In working the plant arranged for the distillation of shale to produce oil, paraffin, and ammonia, a certain quantity of gas is made from the shale, and is used to heat the shale retorts along with an additional quantity of gas derived from the treatment of coal in a vertical shaft or producer, which is combined with each group of eight shale retorts (Fig. 61). The coal shaft is built of brickwork, and is heated externally by the combustion of some of the gas with air heated by waste heat. This combustion takes place in the lower portion of the external space, and the heated products of combustion circulate partly in passages surrounding the shaft, and extending to nearly its top. At the top is a charging-door with a cover, and just below it is the exit-pipe for the gases from the coal. The coal rests on grate-bars, below which there is an ashpit. In the upper portion of the shaft the coal is distilled partly by the externally-

Fig. 61.



Young and Beilby Generator.

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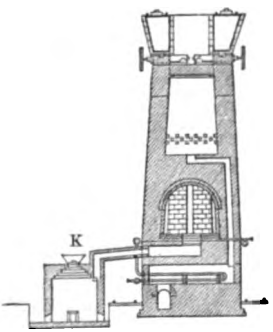
applied heat, and partly by the heat carried up by the hot gases from the combustion in the lower portion. The coke or fixed carbon is consumed by means of air admitted through the grate, combined with a large volume of steam, which is injected into the burning fuel at a short distance above the firebars. The object of the admission of steam in large volumes is to secure and preserve the nitrogen of the coal as ammonia, which necessitates the maintenance of a moderate temperature in the region of combustion. As a further precaution against producing a temperature which would destroy the ammonia, a quantity of the gas which has passed through condensers and scrubbers is returned into the coal-shaft.

In apparatus erected by Mr. Beilby, at Oakbank Oil Works, for the treatment of coal exclusively, a number of vertical retorts are grouped together (Fig. 62) in a "bench" or building containing a double row, with passages surrounding them for circulation of the heating-gases. These are also built of brick, but differ from the coal-shaft in Fig. 61, by having the exit-pipe for the gases at about the middle of its height. The coal is fed in from hoppers at the top, and is distilled at a good heat in the upper half of the retorts. The tar vapours together with steam pass down through hot coke, and are decomposed into permanent gases and ammonia; no tar being found in the pipes when the heats are properly managed. The coke descending into the lower half of the retorts is burned in a mixture of steam and air, with excess of steam in order to secure the ammonia, and the gases from this portion ascend and pass away by the exit-pipe. In starting these retorts it is necessary to obtain some gas for combustion in the chambers or passages surrounding them, in order to heat them up. This gas may be made from coke in the subsidiary producer (K) placed alongside. The gases from the retorts are drawn away by the main, and are passed through condensers and scrubbers, where the ammonia is separated and recovered, as much as from 90 lbs. to 125 lbs. of sulphate of ammonia per ton of coal having been obtained from coal containing nitrogen, equal to 165 lbs. to 170 lbs. of sulphate of ammonia per ton.

Fig. 62.



Generator, Oakbank Oil Works.

Many plans of gas-producers have been proposed during the last

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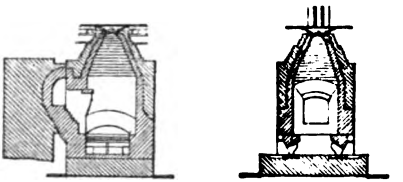
five or six years, which have been either too crude and incomplete, or too costly and complex. The production of water-gas from coke or anthracite has attracted a great deal of attention, from the fact that it has the immense advantage over ordinary producer-gas, of not being diluted with inert nitrogen derived from the air. But a continuous water-gas producer has still to be invented.

Several methods have also been proposed for procuring combustible gas by processes involving other reactions than those of

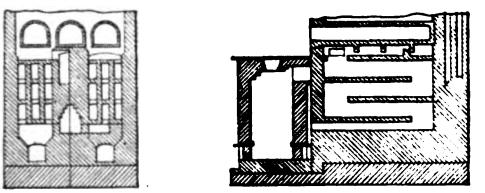
FIG. 63. FIG. 64.



DESSAU OR DIDIER PRODUCER.

the combustion of fuel in air. Among such are the processes of J. F. Parker (1880 and 1881) for heating in retorts a mixture of either carbonate of lime or hydrate of lime with a volatile liquid hydrocarbon. Carbonic oxide and hydrogen are formed, and caustic or quicklime is left, which, when either hydrated or carbonated, may be used over again. A similar process has been patented in the name of Sydney Pitt (No. 2,544, 1883); there is also the process of Mr. E. J. Jerzmanowski for making gas by

FIG. 65. FIG. 66.



SIEMENS ARRANGEMENT AT GAS-WORKS GLASGOW.

carbonizing impure hydrogen produced by means of lime;[1] and the numerous plans for making oil-gas.[2]

The manufacture of oxygen on a commercial scale bears more or less directly upon this subject, because its use in connection with the combustion of fuels may materially modify many heating

[1] The Journal of the Society of Chemical Industry, 1884, p. 560.
[2] *Ibid.*, vols. ii. and iii. 1882 and 1884.

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operations, and greatly influence the methods of producing heating-gas. One or two methods for its production have been proposed, but Mr. Tessié du Motay is the only person who seems to have carried out any process on a large scale.[1]

In connection with the heating of the retorts in gas-works, several forms of producers have been used, which in this case are worked exclusively, or nearly so, with the coke discharged from the retorts. These producers are, however, either modifications of some of the plans described, or have so much in common with them that a separate description is unnecessary. The principal are those of Klönne of Dortmund, of Valon, of Baecker, the Dessau or Didier producer (Figs. 63, 64) used in Dresden, and the modification, or so-called modification, of the Siemens circular producer used in the Corporation Gas-works at Glasgow (Figs. 65, 66). The Klönne construction has also been introduced in one or two gas-works in Great Britain.[2]

There are also the plans of Muller and Eichelbrenner, of Liegel, modified by Mr. G. E. Stevenson of Peterborough, of Dietrich, and of Schilling.[3]

Recovery of Ammonia and Tar from Producer-Gases.

The gases from gas-producers, as well as those from blast-furnaces and from coke-ovens, have been utilized as sources of ammonia, and although results so good as those obtained from the Young and Beilby plant are not to be expected where the primary object is the production of good heating-gas, yet from 20 lbs. to perhaps 30 lbs. of sulphate of ammonia per ton of coal consumed may be recovered.

The only plans hitherto proposed for application to the gas from producers are the ordinary gas-works plant, as applied by Messrs. William Baird and Co. to blast-furnace gases at Gartsherrie, and advocated for application to producer-gases by Mr. W. S. Sutherland, and the system applied by Messrs. R. Addie and Sons to blast-furnace gases at Langloan, and advocated for application to producer-gas by Mr. Alfred Wilson and the Author. The Messrs. Neilson[4] have adopted a slightly different process at

[1] Engineering 1869, vol. viii. p. 255; 1870, vol. x. p. 117. W. Noel Hartley. "Air and its relations to Life," second edition, 1876, pp. 37, 38. The Journal of the Society of Chemical Industry, vol. i. 1882, pp. 59, 338. See also Dingler. Polytechnisches Journal, vol. cc. pp. 466. The Journal of the Chemical Society of London, vol. ix. p. 859.

[2] The Journal of the Society of Chemical Industry, vol. iii. 1884, p. 89.

[3] British Association of Gas Managers. Reports of Proceedings, 1880 and 1881.

[4] Patent, No. 440, 1882.

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Summerlee Ironworks applied to blast-furnace gases, but not as yet to gas-producers.

In general the recovery of ammonia and tar from the gases from coke-ovens is carried out on the plan of condensation and washing with water as in ordinary gas-works; and, if the Jameson coking-process be excepted, in which air is admitted to the interior of the ovens, there is a strict similarity between the quantities and nature of the products treated in these operations, the quantity of gas being, however, rather less from close coke-ovens than from gas-retorts.

In the case of blast-furnaces and gas-producers worked with air, the conditions are, however, very different. In these 1 ton of coal yields 130,000 cubic feet of gas, or more according to the temperature of the gas, weighing nearly 4 tons, and having a temperature of not less than 500° Fahrenheit. This large quantity of gas does not contain on an average more than 7½ lbs. of ammonia (equal to 30 lbs. of sulphate of ammonia), and the difficulty of recovering this quantity may be estimated from the fact that the 10,000 to 12,000 cubic feet of gas from gas-works retorts, and the 8,000 cubic feet of gas from close coke-ovens, are treated for the same quantity of ammonia. In dealing with the gases of blast-furnaces and gas-producers worked with air, or with air and steam, it is thus necessary to cool and manipulate more than thirteen times that volume of gases, with the further disadvantage that the tar from these gases is of comparatively little value. It is therefore obvious that to cool to the same degree the gases from 1 ton of coal from blast-furnaces or gas-producers, thirteen times the cooling-power is necessary as compared with what is required in cooling in the other instances, and this additional outlay for plant is not compensated by the same return in products recovered.

Despite the enormous demands of this method in size and cost of plant, it has been successfully adopted at Gartsherrie under the patents of Messrs. Alexander and McCosh,[1] and similar arrangements have been proposed by Mr. Sutherland.[2]

The process of Messrs. Addie does not involve the cooling of the enormous volume of gases to anything near the same point, and the plant required is much more moderate in dimensions. The process consists in treating the furnace-gases with sulphurous

[1] For a full account of the various processes applied to the recovery of ammonia and tar from blast-furnace gases, see paper by Mr. W. Jones in The Journal of the Iron and Steel Institute, 1885, p. 410.

[2] The Journal of the Iron and Steel Institute, vol. i. 1884, plates iii to vii.

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acid in the state of gas, which is mixed with the furnace or producer-gases at any convenient point. The result is the instantaneous formation of gaseous sulphite and some sulphate of ammonia, which are readily separated by scrubbing with water in vertical scrubbers, in which the water is kept in circulation by pumping.

The arrangement of plant is illustrated in Figs. 67, 68, the pipes for externally cooling the scrubbers being, however, not required in dealing with blast-furnace gases. These were added by Mr. A. Wilson in view of the higher temperature of the gases from gas-producers, in order to prevent the carrying over of steam along with the gases which have subsequently to be burned for producing heat. Mr. Wilson remarks on this subject that "In

FIG. 67.

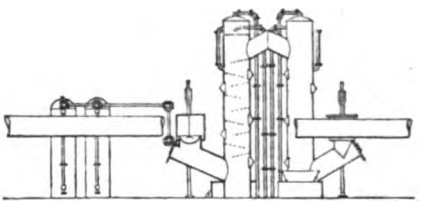
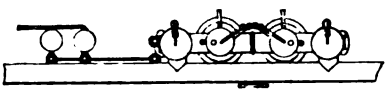


FIG. 68.



ADDIE PLANT FOR RECOVERY OF AMMONIA.

the case of the treatment of gases not exceeding 500° Fahrenheit, every drop of water used is used inside the scrubbers, and comes out as strong liquor. The water pumped in at the top of the scrubbers rapidly rises in specific gravity to any desired strength, even up to 40° or 50° of Twaddell, if necessary, by the solution of salts of ammonia. This liquor is then converted into sulphate of ammonia by distillation with lime in the ordinary way, but it may be employed for many purposes as it stands, without conversion into sulphate, such as, for instance, the production of ammonia for the ammonia-soda process, or for the manufacture of liquor ammonia."[1] The following analysis, on the authority of

[1] The Journal of the Society of Chemical Industry, vol. ii. 1883, p. 459.

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Dr. R. R. Tatlock, of Glasgow, represents the conditions of combination in which the ammonia existed in a sample of crude liquor of 15° Twaddell:—

	Per cent. Weight in Volume.		
Sulphate of ammonia . .	2·57	containing ammonia .	0·66
Sulphite „ . .	3·44	„ „ .	1·01
Hyposulphite „ . .	4·18	„ „ .	0·96
Total ammonia salts .	10·19	„ „ .	2·63

Specific gravity 1·076.

"The sulphurous acid is obtained from sulphurous shale, coal-brasses, or any kind of waste sulphurous materials, but it may also be produced from pyrites." These materials are burnt in a suitable retort by means of a forced current of air, and the sulphurous-acid gas is conveyed by a pipe to the mixing point in the gas-main.

In order to prove whether the whole of the sulphur thus introduced into the gases was arrested in the scrubbing operations, the gas at Langloan from the flue beyond the scrubbers was tested for sulphur by Dr. Clark, with the following result:—

Sulphur per cubic foot of gas	0·1 grain.
„ „ ton of coal burned	1·85 lb.
„ „ cent. of coal	0·08

"From this it will be observed," remarks Mr. Wilson, "that the sulphur in excess is only a mere fraction—probably not more than one-tenth—of the amount which must exist originally in the coal itself," so that for metallurgical purposes the quality of the gas as regards the sulphur from the coal is improved by this process of treatment.

The Paper is accompanied by numerous photographs and drawings, from which the Figs. in the text have been engraved.

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APPENDIX.—ANALYSES

No.	Producer.	Locality, &c.	Percentage Composition.					
			CO.	H.	CH_4 &c.	O.	CO_2.	N.
1	Ebelmen	? Audincourt	33·3	2·8	..	..	0·5	63·4
2	,,	,,	27·2	14·0	..	..	5·5	53·3
3	,,	,,	32·4	10·26	..	..	7·23	50·1
4	,,	,,	18·98	17·85	..	..	13·2	49·97
5	,,	,,	21·83	7·64	..	..	9·06	61·47
6	,,	,,	33·53	1·48	..	..	0·85	64·1
7	Thomas and Laurens	France	33·5	2·8	..	..	0·5	63·4
8	,,	,,	27·2	14·2	..	..	5·6	53·2
9	Siemens	St. Gobain	24·2	8·2	2·2	..	4·2	61·2
10	,,	Paris Gas-works	26·0	..	..	0·5	4·5	67·5
11	,,	Dowlais	25·6	..	4·4	..	4·3	65·7
12	,,	,,	24·4	8·6	2·4	..	5·2	59·4
13	,,	,,	24·92	..	3·62	..	6·95	64·5
14	,,	,,	25·97	0·65	1·45	..	8·71	63·2
15	,,	Midvale, Philadelphia	15·4	8·3	4·7	..	5·7	65·9
16	,,	,, ,,	23·6	6·0	3·0	..	1·5	65·9
17	,,	,, ,,	17·7	9·8	2·4	..	5·9	64·2
18	Ponsard	France	21·0	12·0		..	6·0	61·0
19	,,	,,	25·0	11·0		..	4·0	60·0
20	,,	,,	22·0	21·6		..	4·4	52·0
21	,,	,,	22·5	16·0		..	4·5	57·0
22	,,	,,	24·0	17·0		..	4·0	55·0
23	,,	,,	21·0	..		..	11·0	..
24	,,	,,	23·0	..		..	9·0	..
25	Wilson	Middlesbrough	26·89	11·55	1·45	..	4·0	56·11
26	,,	,,	23·41	13·82	2·22	..	4·69	55·86
27	,,	Jemmapes near Mons, Belgium	23·60	10·55	3·05	..	5·25	57·55
28	,,	Wrexham	19·83	12·15	3·61	..	7·14	57·24
29	,,	,,	26·48	12·42	5·91	..	4·11	51·06
30	,,	,,	23·98	14·68	4·72	..	6·26	50·36
31	,,	,,	29·58	0·90	0·91	..	6·91	61·7
32	,,	,,	26·33	1·11	1·43	..	8·29	62·84
33	,,	,,	26·89	11·55	1·45	..	4·00	56·11
34	Philips	Philadelphia	27·30	..	1·4	..	3·9	67·4
35	,,	,,	20·0	8·7	1·2	..	8·7	61·4
36	,,	,,	16·5	8·6	2·7	..	9·3	62·9
37	,,	,,	16·0	15·3	1·9	..	7·5	59·3
38	,,	,,	15·5	14·9	..	..	8·0	61·6
39	,,	,,	22·3	28·7	1·0	..	6·1	41·9
40	Kidd	London	28·6	14·6	..	..	4·0	53·0
41	,,	,,	22·6	10·0	4·9(?)	..	4·5	58·0
42	,,	,,	26·4	13·5	1·4	..	3·9	54·8
43	Dowson (approximate analysis)		30·0	20·0	..	..	3·0	47·0
44	,,	London	22·2	19·3	..	..	5·8	52·7
45	,,	,,	25·07	18·73	0·62	0·03	6·57	48·98
46	Young and Beilby	Midcalder	8·1	28·6	2·3	..	16·6	44·4
47	,,	,,	10·72	37·19	..	..	21·32	30·77
48	Dawes	.. ..	31·86	54·52	1·62	..	12·00	..
49	,,	.. ..	29·30	56·90		..	13·80	..
50	Lowe	Manayunk, U.S.A.	20·04	77·03		0·20	2·29	..
51	,,	Baltimore	21·51	46·49	27·50	0·20	..	4·30
52	Strong	New York	35·88	52·76	4·11	0·77	2·05	4·43
53	Retort-made gas	Manchester	8·88	46·2	39·11	0·65	3·01	2·15
54	Blast furnace.	Scotch	29·24	0·9	..	..	21·7	48·2
55	,,	Askam	33·80	0·14	..	..	13·47	52·59
56	,,	Cleveland	27·03	0·06	..	..	14·32	58·54
57	Bessemer converter		31·11	2·0	..	..	1·34	65·55

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of Producer-Gases.

Percentage of Combustible.	Calories per 100 Litres at 0° Cent. × 760 Millimetres.	Cent. lb.-units produced. Combustion under constant pressure.	Theoretical Maximum Temperature. Degrees Centigrade.	Authority.	No.
36·1	..	..	..	Ebelmen.	1
41·2	..	..	..	,,	2
42·66	..	..	..	,,	3
36·83	..	..	..	,,	4
29·47	..	..	..	,,	5
35·01	..	..	..	,,	6
36·3	..	..	..	Kraus.	7
31·4	..	..	..	,,	8
34·6	..	..	..	,,	9
26·0	..	..	..	Felix Leblanc.	10
30·0	..	..	..	G. J. Snelus.	11
35·4	..	..	..	,,	12
28·54	..	..	2,000	,,	13
28·07	..	..	2,055	,,	14
28·4	116,753	..	1,656	Mag. Troilius.	15
32·6	114,939	..	1,761	,,	16
29·9	103,843	..	1,642	,,	17
33·0	..	..	..	Syl. Périssé.	18
36·0	..	..	..	,,	19
43·6	..	..	..	,,	20
38·5	..	..	..	,,	21
41·0	..	..	..	,,	22
..	..	..	..	,,	23
..	..	..	..	,,	24
39·89	..	..	..	Pattinson and Stead.	25
39·45	..	..	..	,, ,,	26
37·20	..	..	..	Director of Glass-works at Hainault, Roux.	27
35·59	..	..	..	J. H. Darby.	28
44·81	..	..	..	,,	29
43·38	..	..	..	,,	30
31·39	..	1,139	2,128	J. E. Stead.	31
28·87	..	1,201	2,204	,,	32
39·89	..	1,136	2,100	A. Wilson.	33
28·7	93,966	..	1,619	Mag. Troilius.	34
29·9	97,184	..	1,658	,,	35
27·8	99,074	..	1,575	,,	36
33·2	11,474	..	1,706	,,	37
30·4	92,620	..	1,613	,,	38
52·0	164,164	..	1,846	,,	39
43·2	..	..	..	T. W. Keates.	40
37·5	..	..	..	S. W. Davies.	41
41·3	..	..	..	,,	42
50·0	..	..	2,268	Prof. Ayrton.	43
42·0	..	..	..	H. L. Greville.	44
44·42	..	..	..	W. Foster.	45
39·0	..	..	..	G. T. Beilby.	46
47·91	..	..	..	,,	47
88·0	..	..	..	Dr. Percy.	48
86·2	..	..	..	,,	49
97·07	..	..	..	Dr. H. Wurtz.	50
95·5	..	..	..	,,	51
92·75	..	..	3,028	Dr. G. E. Moore.	52
94·19	..	..	..	Bunsen.	53
30·14	..	946	2,010	I. L. Bell.	54
33·94	..	859	1,873	W. Crossley.	55
27·09	..	669	1,917	J. E. Stead.	56
33·11	..	..	..	G. J. Snelus.	57

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All these analyses are stated in percentages by volume, except Nos. 29, 30, 48, 49 and 50, which are percentages by weight. No. 31, however, gives analysis 29 in percentage composition by volume:—

No. 1 is the mean of three analyses of gas from wood charcoal used in the Ébelmen producer with dry air.

No. 2 is analysis of gas from wood charcoal with air and steam used in the producer.

No. 3 is the mean of two analyses of gases from wood.

No. 4 „ „ three „ „ „

No. 5 „ „ two „ „ peat or turf.

No. 6 „ „ „ „ „ coke.

The absence of carburetted hydrogen from these analyses is said to be due to the method of analysis adopted by Ébelmen.

No. 7. From a generator worked by Thomas and Laurens with air only and coke.

No. 8. From the same generator with steam added.

No. 9. Gas made from a mixture of three-fourths caking and one-fourth non-caking coal. Temperature of gas issuing from producer 1,100° Fahrenheit.

No. 10 is from a producer using coke at the Paris Gas-works.

No. 11. Gas from ordinary Siemens producer worked by draught.

No. 12. Gas from producer worked with closed ash-pit and air-blast produced by steam-jet.

Nos. 15 and 17 are samples of gas taken just after cooling down fires with water from below.

No. 16. Sample taken just before cooling down.

No. 18. From a coal containing 21 to 19 per cent. volatile products and 79 to 81 per cent. Coke estimated without ash and moisture. Ash being 9 to 18 per cent. About one-fifth of cinders from other coal were charged with the coal, and the air supplied to the producer was cold though the producer was worked too fast and too hot.

No. 19. Three-sixths of a good gas-coal and two-fifths cinders from other fires were charged. Temperature of gas issuing from producer 650° Centigrade.

No. 20. From a producer worked at high temperature supplied with air heated by a recuperator and slightly moist. The coal used was gas-coal containing 65 per cent. of a feeble coke and 35 per cent. volatile matter. Ash 15 to 20 per cent. Temperature of gas 850° Centigrade.

No. 21. From a forge coal producing 73 per cent. Coke of good quality, and 27 per cent. volatile matter. Ash 20 per cent. No cinders charged. Water was allowed to trickle under the grate. Temperature of gas that of melting copper.

No. 22. Same conditions as in No. 18, but with dry air.

Analyses 16 to 20 were made by means of Orsat's apparatus.

No. 23. From turf or peat, very moist, containing 50 per cent. of water, a little of which may have escaped while the heat was in boxes prior to charging.

No. 24. Moist peat containing 28 per cent. water.

The analysis of the peat used in 21 and 22 gave after dessication—

Volatile matter	58·5 per cent.
Fixed (including ash)	41·5 „
	100·0

Ash = 8 per cent.

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No. 25. From producer using Durham coal.
No. 26. Average of six samples of gas taken during an hour, producer working on a fine Yorkshire slack.
No. 27. Producer using Belgian coal.
No. 28. Coal used, refuse. Pressure of gas in producer 20 millimetres. Temperature of fuel in producer 1,300° Centigrade. Temperature of gas in producer 960° Centigrade, of gas above valve where admitted to furnace 740° Centigrade.
Nos. 29 and 30. Coal used, best coal. Pressure of gas 22 millimetres. Temperature of gas in producer 600° Centigrade, at furnace 450° Centigrade. The level of the fuel was kept 2 feet higher in these cases than in No. 26, and a larger amount of steam was introduced by the steam-jet at a lower velocity.
No. 31. Producer using Tees Hatton coal. Composition of gas by weight.
No. 32. „ „ Park Coal Co., slack „ „ „
No. 33. Same analysis as No. 29 stated in composition by volume.
No. 34. Producer worked with fan blast and working hot.
No. 35. „ „ steam „ „ „ cold.
No. 36. „ „ „ „ Sample taken just before cleaning producer. Coal bad.
No. 37. Steam blast; bad coal.
No. 38. „ „ normal working.
No. 39. Sample taken ten minutes after No. 36, with air shut off as far as possible and steam only admitted.
No. 40. Gas made from peat charcoal.
Nos. 41 and 42. Kidd's generator worked with steam and air and anthracite.
No. 43. Gas made from anthracite. Approximate analysis only.
No. 44. Dowson's generator worked with steam and air and Trimsaran anthracite.
No. 45. Similar generator with Garnant anthracite.
No. 46. Air-supply rather excessive.
No. 47. Gives corrected analysis with air-supply regulated so that half the fixed carbon should be consumed with air and half with steam.
Nos. 48 and 49. Gas resulting from the action of steam on red-hot coke.
Nos. 50 and 51. Water-gas from anthracite with addition of petroleum to enrich the gas for illuminating purposes.
No. 52. Water-gas, with addition of powdered coal.
Nos. 53, 54, 55, and 56, are added for comparison.
No. 57 is the average composition of the gas escaping from the Bessemer converter during the latter half of a "blow."

[Discussion.

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Discussion.

Sir Frederick Bramwell.

Sir FREDERICK BRAMWELL, President, observed that the Paper dealt with a subject of the highest importance, viz., the economical use of fuel, and the possibility of obtaining from it products which were of themselves of great value. It was an exhaustive Paper, and he hoped that the members would, by fully and thoroughly discussing it, prove that it had not been given to an unappreciative audience.

Mr. Head.

Mr. JOHN HEAD desired to speak not so much for himself, but as representing Mr. Frederick Siemens, and formerly Sir William Siemens. He had been startled by the theory developed in the Paper. During all the time he had been with Sir William Siemens and his brother—nearly twenty-six years—he never heard of the proposition to obtain a higher calorific intensity from carbonic oxide made in a gas-producer than that obtained from solid coal; but the Paper appeared to lead to that conclusion, and he had endeavoured to ascertain how the error had been made by the Author. In looking at the equations he found that they were taken directly, or indirectly, from the standard treatise on Fuel by Dr. Percy. They were therefore unquestionable as to their accuracy; but the Author had misapplied one of them. He had considered carbonic oxide as made in a gas-producer, and had dealt with it as if it were pure carbonic oxide without nitrogen. By making the necessary alteration in the equation in order to bring in the nitrogen, the value 2,980° Centigrade would be reduced to about 1,880°, showing a difference of 1,100°, or a loss due to the conversion of solid carbon into gas of about one-third its calorific value, instead of a gain of 265°, or about 10 per cent. as supposed by the Author. It was necessary to mention these facts, because, in considering the application of producer-gas, it was as well to clear the ground of any illusion which might be entertained on the subject. Sir William Siemens and Mr. Frederick Siemens always had in view the loss of effect referred to, and they had therefore attached great importance to the proper combustion of gas, and to the use of regenerators, in order to derive all possible advantages from the combustion of carbonic oxide. These subjects were, however, beyond the scope of the Paper, and he would not enter into their consideration at the present time. The Author had taken great trouble to bring forward many forms of gas-producers, particularly those which

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had preceded the Siemens producer; but he had omitted two forms of the Siemens producer which had been introduced, one by Messrs. William and Frederick Siemens in 1863, and the other by Sir William Siemens in 1864, which included most of the features that had been referred to under the names of Minary, Wilson, Gröbe, Lürmann, and others. The Author stated that Sir William Siemens in his patent of 1883, No. 663, had reproduced features which were contained in the Wilson producer; this was a misapplication of facts. Sir William Siemens, in the producer referred to, had no doubt adopted some features contained in his invention of 1863, which Mr. Wilson had also incorporated in his producer, dating from 1876, and this had led to the misstatement. The Lundin arrangement for cooling producer-gas by means of a shower bath had been previously introduced by Sir William Siemens and Mr. Frederick Siemens, in 1863; but the more simple and efficient cooling-tube, which was already in use and found to answer every purpose, was preferred in all cases, except where a pressure of water was available and iron tubes were costly. The cooling of gases was a necessity wherever fuel containing moisture had to be employed. Only last week he had designed several producers and furnaces for America, and as the fuel contained 9 per cent. of moisture, he put in cooling-tubes, although the Siemens new-type producer was to be used. In cases also where wood or peat had to be employed, the cooling of gases was an absolute necessity. Messrs. Siemens had furnaces in Ireland, working with peat as fuel, which would not have answered without cooling-tubes. They had also some furnaces in Italy working with peat, concerning which the same remark might be made. The condensation of water vapour was an important function of the cooling-tube which had completely escaped the notice of the Author. With reference to the Siemens new-type gas-producer, the Author stated that it was worked by chimney-draught. But this was not so. All the Siemens producers were worked by pressure; and as that pressure was obtained in the Siemens old-type producer (Fig. 69) without a forced air-supply, it was still easier to obtain it in the Siemens new-type producer (Fig. 70) in which a steam-jet was used. Hundreds of the new-type producers were working very economically with all sorts of fuels. The analyses, given by the Author to show the quality of gas made, might be dismissed as practically valueless; for analyses were only comparable with different gas-producers working on the same coal; moreover, he could give analyses of gas made in the Siemens new-type producers showing much better results than those selected by the

Mr. Head.

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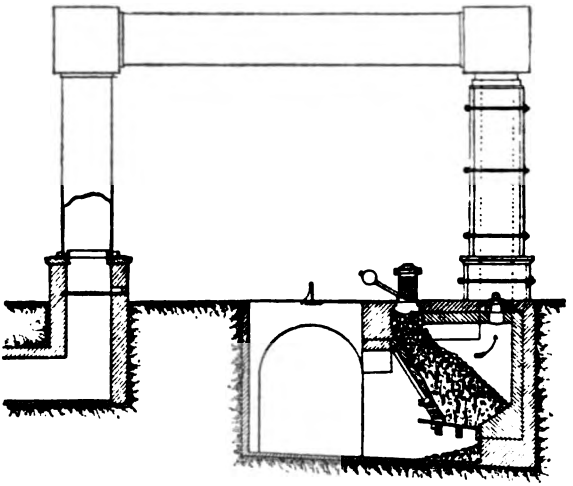
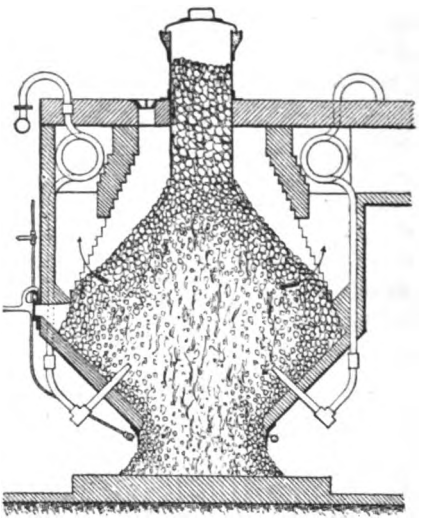
Mr. Head. Author, which were all of gas made in the old-type producer;

FIG. 69.



SIEMENS OLD-TYPE PRODUCER.

FIG. 70.



SIEMENS NEW-TYPE PRODUCER.

but a proper criterion as to the value of a producer was to work two of them with the same kind of fuel, and to do the same work.

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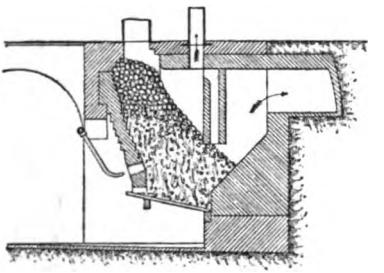
He had done this with regard to the Siemens new and old-type gas-producers, and the saving of fuel effected, the same kind of coal being used, was 28½ per cent. This had been carried out since the death of Sir William Siemens, in order to establish the practical value of the new-type producer. The Siemens modified old-type gas-producer (Fig. 71) was introduced by Mr. Frederick Siemens, with a view to utilize the many thousands of producers that existed of the old type, for the production in them of gas rich in hydrocarbons, as was done in the Siemens new-type producer, in which there was a decomposition of the hydrocarbon gas liberated from the freshly-charged fuel, by bringing it into contact with incandescent fuel, and that decomposition resulted in the formation of permanent gases which otherwise would go to form tar. The same effect was obtained by passing the gases through tubes or other heated channels in the brickwork, but, contrary to Mr. Head.

FIG. 71.



MODIFIED SIEMENS OLD-TYPE PRODUCER.

the Author's assertion, the production of soot was not greater in this than in the other forms of producers. By making the curtain-wall solid, and so affording an opportunity of taking away the hydrocarbon gases liberated, in order to extract ammonia from them, it was intended to adapt the invention to an additional purpose. The Author was afraid if that should be done a double current would be set up, which would interfere with the action of the producer; but no trouble of this kind need be apprehended, for as the hydrocarbon gases would be taken away by means of an exhauster, which would be under complete control, its action could be regulated so as to take only, or perhaps rather less than, the volume of hydrocarbons liberated in order to maintain a pressure in the producer. If a cooling-tube were employed, the absence of hydrocarbon gases would not interfere with its action, but would promote the draught, contrary to the

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Mr. Head. Author's expectations. If there were two vertical columns of gas at different temperatures, one of which contained hydrocarbons in the gaseous but condensable state, when those hydrocarbons reached the cooling-tube connecting the two columns they would be condensed into tar. Thus a vacuum would be caused, not an increase of density; but what was relied upon to create a flow of gas towards the furnace was an increase in its density, and that increase was obtained simply by lowering its temperature. By lowering the temperature of the gas from 273° to 0° Centigrade the density would be doubled; in other cases the increase in density would be in the same proportion. With regard to the Healey producer, as the Author had shown in his Paper, round producers had existed long before Mr. Healey mentioned them, and Messrs. Siemens attached no importance whatever to the outward shape of a producer. The difference between the Wilson producer and the Siemens producer was that the former was worked at a much higher pressure of blast. In 1863 Messrs. Siemens had constructed and worked producers with closed ashpits and injection of air by means of steam-jets, and the invention comprised other features copied in the Wilson producer. The objection to high pressure of blast, as in the Wilson producer, was that the air-currents were likely to take certain channels inside the producer, and when that occurred a large volume of carbonic acid would be produced and pass with the gas to the furnaces. It would be difficult to prevent this occurring frequently. A further objection to high-pressure blast was that the stoppage of a producer for cleaning, or through derangement in working, necessitated providing a spare one to be held in reserve; if this was not done great difficulty was experienced, and in the other case fuel would be wasted, while the gas in the spare producer would not be required. The advantage of the Siemens gas-producers with open ashpits was that as soon as the production of gas diminished the introduction of stoking bars into them improved their action; this improvement went on while the producers were being clinkered and cleared from ashes; and this remark applied to all the forms of Siemens gas-producers, namely, the old-type, the new-type, and the modified old-type. That was the main difference between the action of the Siemens and the Wilson producers, and it was entirely in favour of the former.

Mr. Dowson. Mr. J. Emerson Dowson said that in this historical sketch of gas-producers the Author had made no reference to the apparatus invented by Messrs T. and W. Kirkham in 1852–4, which was a most important departure in the working of gas-pro-

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ducers, and which, he thought, deserved to be mentioned. Before that time all gas-producers had been heated by external fires; but Messrs. Kirkham dispensed altogether with external fires. They put fuel into the generator and lighted it. Next they turned on a blast of air through the fuel, and so brought it to a high temperature. They then shut off the air and let in steam, which was readily decomposed, and a gas of considerable power was produced. After about fifteen minutes the temperature was so reduced, owing to the chemical reactions which occurred, that the admission of steam was stopped and the air-blast was let in again. The effect was, that the whole process was carried on by internal combustion, there was no need of external firing, and that, of course, considerably diminished the cost of working. It also prevented the oxidation of the retort, and there were several other advantages connected with it. True, this involved intermittent working, but so did every system in which air was excluded during the process of decomposition. Since that time all gas-producers had been fired internally, whether working intermittently or continuously, and great merit was certainly due to the inventors of the system. In referring to the Tessié du Motay apparatus, which he thought had been too slightingly spoken of, the Author had made no reference to its use on a very large scale by the Municipal Gaslight Company of New York. He had seen the apparatus, and had been struck with its simplicity, and the workmanlike manner in which it was handled. It certainly deserved to be ranked among the successful gas-producers of the intermittent type. The Author had claimed for Mr. Wilson the novelty of injecting air into a producer by means of a steam-jet; but on the previous page of his Paper he had already mentioned that Mr. Kidd used the same device for his own little generator; in fact there were many instances in which that had been done. The Author had described that which Mr. Howson was now proposing to do, in passing steam and air downwards through the fuel, instead of upwards in the usual way. He might mention, however, that Mr. Harkness, of the United States, took out a patent in England for such a system of working in 1874, and tried it experimentally. The inventor's idea was, that by passing the steam and air downwards, he would ensure a better distribution of them throughout the whole sectional-area of the generator, and so cause them to come better into contact with the fuel. In practice, Mr. Dowson believed the method did not answer very well. Nor could he agree in the necessity for it on these grounds, seeing that in the working of all gas-producers, the gas was at a pressure

Mr. Dowson.

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Mr. Dowson. above that of the atmosphere, and that alone, he considered, should be sufficient to cause the steam and air to spread across the generator if it were properly proportioned. The Author had referred to the production of oxygen gas, and he might therefore be permitted to mention an apparatus introduced by Messrs. Brin Brothers, of Paris, which was shown on a full working scale at the International Inventions Exhibition, and had also been fully described at the meeting of the British Association at Aberdeen. In that apparatus, the inventors had carried out in a new way the well-known process of using barium oxide to take up oxygen from the air, forming barium peroxide, which was afterwards reduced to barium oxide with the evolution of oxygen gas. The apparatus was probably the best of its kind, and was deserving of mention. He could not agree with Mr. Head that the analyses given by the Author were valueless. He considered that they enabled an opinion to be formed as to the merits of each producer. No doubt they varied very much, as might naturally be expected, because the producers were worked under such very different conditions with regard to fuel and other matters. It was to be regretted, however, that the Author had not been able to supply some account of what each producer was used for, and with what results; because upon those results more than upon any gas analyses depended the commercial value of the producers themselves.

Mr. Bauerman. Mr. H. BAUERMAN remarked that the Paper was such an encyclopædic one, that to discuss it minutely would require more time than he should like to occupy. He might, however, be permitted to direct attention to one or two points in which he differed from the Author. In speaking of the history of the gas-producer the Author had stated that "The employment of waste gases from iron-furnaces and other metallurgical operations was clearly the first movement in this direction." He could not help thinking that the gas-producer was a great deal older than that. Gas furnaces had existed long before they were so-called. There was the old hollow fire for re-heating the tin-plate forges, and also used in Sheffield, which was employed at a very early period—early in the eighteenth century, and perhaps before that time. There was also the South Wales reverberatory furnace, whose fireplace with the clinker grate was essentially a gas-producer. Indeed, further back it was well known to very primitive people that deep columns of fuel gave off inflammable gas, which would have to be dealt with in some way. It was interesting to watch how, when a little iron furnace in India, 5 or 6 feet high, was newly lighted, the native smelters standing some distance to

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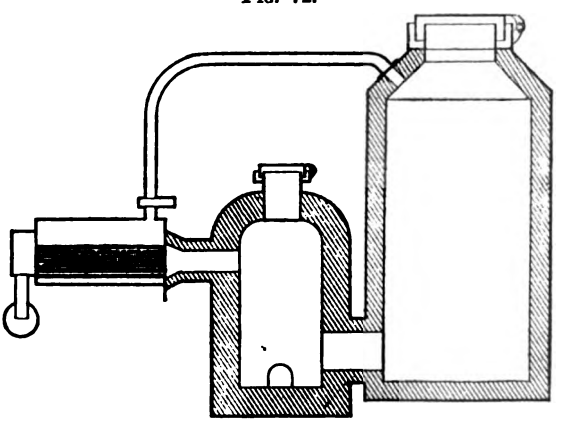
windward fired the gas before they approached the furnace top, knowing that the gas coming off was poisonous. It was, of course, difficult to discuss the question completely without considering the method of burning the gas, as so much turned upon how the gas was used, and for what purpose. He thought that many of the producers were not likely to be very economical. They were no doubt useful appliances for getting large masses heated, and obtaining a flame of very high temperature. But, if the work was done on a large scale, as in the case of a blast-furnace working at its best, such a furnace did its work, and gave off gas at the end, substantially the same, and nearly as good for heating-purposes as the ordinary producer-gas. That, he imagined, was due to the fact that greater economy was obtained by working the furnaces quickly. With the slower-working producers, with the large mass of material round about them, they were pretty much in the condition of the old blast-furnaces; there was probably no very great intensity of radiation, but there was a great mass of radiating material as compared with the fuel inside, and there would be a considerable dissipation of heat from that cause. There was one kind of gas-furnace, the Boetius zinc-furnace, which the Author had not noticed. It was largely used in Belgium and on the Rhine for heating zinc retorts. It was a very simple form of gas-furnace, and was found in working to be one of the best. Mr. Bauerman.

Mr. E. A. Cowper described an arrangement for producing a Mr. Cowper.

Fig. 72.



Cowper Producer.

mixture of carbonic oxide and hydrogen, by the decomposition of steam in contact with carbon (Fig. 72). It was a regenerative

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Mr. Cowper. hot-blast stove, made hot by burning gases. The steam was brought to the top of the stove, and passed down through it. After this it entered into the chamber which was charged with coke at a full red heat, and having been previously heated to 1,500° Fahrenheit in its progress through the stove, it kept the coke red-hot without introducing any air. The gas was cooled by the boiler, and then entered the hydraulic main. That method was invented by him in 1857, so that there was a continuous arrangement for making carbonic oxide and hydrogen from incandescent fuel. The regenerative furnace was invented by Mr. Frederick Siemens, who took out his patent in 1856.

Mr. Crampton. Mr. T. R. CRAMPTON said he had not given much attention to the burning of oils; he had done more in the burning of dust-fuel, which was a great success in regard to economy and regularity of high temperature, wrought-iron having been easily melted. A weight of 800 lbs. of puddled ball had been melted in forty minutes without being oxidised; but no refractory material for lining the furnace, combined with the fluxing action of the lining, could be found to withstand the heat produced. He, with Mr. W. Anderson, M. Inst. C.E., during the past year, made a few experiments with a boiler, trying to avoid the use of a casing of refractory material, but without any great success. Upon resorting to a lining composed of the best refractory material the result was good as regarded combustion, but the lining was so rapidly destroyed as to render it of no practical use. There were several means which might be made to answer the purpose, one method being the employment of self-made slag, but that had not been fully tried.

Mr. Siemens. Mr. ALEXANDER SIEMENS desired to direct Mr. Bauerman's attention to the new producers of Mr. Frederick Siemens, which had been mentioned by Mr. Head. They were constructed with the object of having as small a radiating surface as possible with regard to the amount of fuel consumed. These producers had about four times the area of the old ones, and while in the old ones about 2 tons of coal were burned in twenty-four hours, the new ones could burn 15 tons, which was about double the amount for the same grate-area, and of course that showed the combustion to be quicker than before.

Mr. Foster. Mr. WILLIAM FOSTER said that the quotations of the Author with reference to himself, though somewhat brief, expressed exactly that which he had intended at the time when he spoke on the subject. Mr. Foster was dealing with producers, generally, while the Author dealt with them specifically. He had in view, when

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speaking, no less than three methods for the recovery of the nitrogen of coke as ammonia. One was the lime-method proposed by Mr. Wanklyn; another was the treatment of the coke by means of hydrogen got in a certain way; and the third was the treatment of coke with air and steam. Of these three the method of converting the coke into gaseous-fuel seemed to be scientifically the most promising; it lent itself better to the utilization. He did not know that any producer now in the market was being worked for the recovery of the nitrogen of coal. As to the recovery of by-products, that had been purely incidental to a particular industry. Whether dealing with coke-ovens, or blast furnaces, or the distillation of shale in paraffin-works, engineers had to consider the by-products as purely incidental to the transaction. As far as he knew, no system of treating coal for the recovery of by-products alone was successful. He had recently been told that of the works established during the past three or four years only one now existed. They had been a considerable loss to the proprietors; and the existence of one of the works might be explained by a consideration of matters connected with Stock Exchange operations. The opinion he had expressed with reference to nitrogen of coke being recovered as ammonia he still maintained—that it could only be recovered by the conversion of the coke into gaseous-fuel. He greatly regretted that since he read his Paper before the Institution[1] misfortunes had attended the efforts of those who had tried to work the residual question. "The times were out of joint" with reference to this matter; and the low prices attending the sale of the residuals might be regarded as accounting for the amount of caution exercised by those who had to deal with the treatment of large quantities of coke or coal. He did not see how it was possible to utilize the nitrogen of coke except by a gas-producer, using air and steam; and among the places which were likely to take advantage of such facts as had been brought forward in abundance during the past three or four years, he did not think that there were any better adapted for the purpose than the gas-works of the country. It was only the largest gas-works that could carry on such operations with profit. It could only pay the large gas companies who were already using gaseous-fuel. He admitted it was problematical whether they could utilize the nitrogen of coke and turn it into profit; but they were more likely to solve the problem than persons engaged

Mr. Foster.

[1] "Experiments on the Composition and Destructive Distillation of Coal." Minutes of Proceedings Inst. C.E. vol. lxxvii. p. 162.

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Mr. Foster. in small works, using only a few tons of material a day. It was a question purely of profit and loss. He had no experience with producers, except with that of Mr. Dowson. Mr. Dowson had in view the keeping out of all by-products. He wished to obtain as high a quality of fuel as possible from the coke; the by-products were never considered other than as impurities in the gas. He regretted that the Author had not been able to bring forward some accounts of experiments, having for their object the recovery of ammonia in the process of making gaseous-fuel. That led him back to the point he had before raised—that with the production of more gaseous-fuel, using the ordinary coal-gas as gaseous-fuel, a great quantity of coke, which was often a drug, was incidentally made. The only way to find a use for some of it was to convert it into gaseous-fuel at the works, and it was problematical whether such gas could be scrubbed and washed and ammonia got out of it at a profit. It was now, he thought, pretty well established that gaseous-fuel could be used in many special circumstances. It was of course absurd to say that coke or anthracite could be gasified, and then the total heat-value be got that could be obtained by burning coal or anthracite direct. It was simply a question of getting a greater percentage available for certain purposes. At the best only some 70 per cent. of heat-value of the original coal could be realized, but of that 70 per cent. a still greater percentage could be rendered available in certain industries. The paraffin industries of Scotland, he thought, afforded the best example of works which were increasing their yield of ammonia from coal. There was no doubt that the increase of ammonia had been material during the past few years; but whether the quantity was very great he was not prepared to say.

Mr. Cowper. Mr. E. A. Cowper wished to notice a statement of the Author, to the effect that "heat produced by the combustion of the carbon was stored up in brick-work, which being made white-hot was then used to decompose steam," and also the remark that "the steam became very highly heated, and partially, if not wholly, decomposed in contact with the hot bricks." It was a mistake to suppose that steam passing over hot bricks could be decomposed. If coke was present it took up the oxygen, and formed carbonic acid or carbonic oxide, and the hydrogen was set free, but steam would not be decomposed by the simple application of hot bricks. On the contrary, if hot hydrogen and hot oxygen were brought together, they were in the best possible condition to burn, and form water; he had the authority of some of the best chemists to support this fact.

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Dr. H. E. ARMSTRONG said that the Author had described a large number of producers, but he did not say, in the first instance, why producer-gas was of special value, and he had given no data wherewith to judge of the special advantages to be derived from the use of the various forms to which he had referred. It was important to know, in the first place, what were the advantages to be derived, and, in the second place, how the producer should be worked to suit the various circumstances that cropped up. There could be no doubt that the Siemens family, to whom reference had been made in the Paper, were the true appreciators of the system. In recent Papers read before the Society of Chemical Industry, Mr. Frederick Siemens had brought forward points which it was obvious from the Author's Paper had not been duly appreciated, but which marked a distinct departure in regard to the question under discussion. It was well known that Mr. Frederick Siemens had materially modified the construction of his furnaces—that he had enormously increased the combustion-chamber with the distinct object of keeping away the hot gases from the surfaces to be heated, and also with the object of being able to fully utilize the radiating power of the flame. Mr. Siemens had had the courage to disregard theoretical notions on the subject altogether. He had pointed out that he did not admit that the limit of temperature which dissociation was supposed to involve should be regarded as so important an element as had hitherto been the case, and he, for one, fully shared Mr. Siemens' opinions on that point. He had drawn attention to the enormous importance of getting as much of the heat as possible, not by contact, but by radiation, and that was a very noteworthy point in connection with this discussion. If the matter were settled in Mr. Siemens' favour, engineers would have to pay much more attention than they had hitherto done to the character of the gas produced. Up to the present time the effort had been to completely convert the fuel into combustible gases—into carbonic oxide and hydrogen. Mr. Siemens had, however, pointed out that such a mixture of carbonic oxide and hydrogen was by itself comparatively inefficient. It had no radiating power; it could heat only by conduction. It was deficient in radiating power in consequence of the absence of solid particles. Mr. Siemens had also shown that the most efficient mode of making the flame powerfully radiating was not to cause it to heat surfaces capable of radiating, but to disperse through it carbon particles which were capable of radiating; and that being the case, it was most important not to produce a mere mixture of carbonic oxide and Dr. Armstrong.

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Dr. Armstrong. hydrogen—which was incapable when burnt of giving rise to the production of incandescent solid particles, but to introduce into the producer-gas a certain quantity of hydrocarbons, which would undergo decomposition, and the carbon being separated at a very high temperature would necessarily be heated to incandescence, and would radiate powerfully. This was a point having a most important bearing on the question of the mode in which coal should be burnt, and also on the utilization of by-products. He had been struck from time to time, when he had been at ironworks and had seen the boilers heated by the waste gases, by the enormous volume of flame which was used; and it was well known what great difficulties stood in the way. If the views of Mr. Siemens were correct, great advantage ought to be derived from the mere introduction of a small quantity of residual tarry matter with the combustible gases. If that were done it would supply the radiating element which was naturally absent. With regard to the question of the recovery of by-products, he was inclined to think that the simple form of furnace which had been described ought, if properly worked, to furnish a fair solution of the problem. It ought to be possible, as Mr. Head had pointed out, so to adjust the time during which, and the rate at which, the products were withdrawn from the upper chamber, as to take off practically all that was worth taking off, and he could well imagine that the products might be condensed and portions utilized, and that the residue might be returned for use in the way he had suggested. As to the recovery of nitrogen from the coke, he had stated during the discussion on Mr. Foster's Paper,[1] and he now desired to state still more positively, that he did not think it was worth while to recover nitrogen from coke. Mr. Foster had said that he was not aware that there was any producer in the market which would render that recovery possible. Dr. Armstrong thought there never would be one. The quantity was so small, and the conditions under which coke must be burned in order to recover the nitrogen as ammonia were of such a kind, that it would never answer. The coke could not be burned effectively to produce gaseous-fuel and at the same time to produce the maximum quantity of ammonia. The two things were incompatible. To produce the maximum quantity of ammonia as low a temperature as possible was required: the ammonia should be given off at a low temperature; it should be highly diluted; and there should be no trace of oxygen, but an excess of water vapour. Such conditions

[1] Minutes of Proceedings Inst. C.E. vol. lxxvii. p. 191.

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were incompatible with the production of good producer-gas. Whenever it came to be important to make ammonia on a large scale, he had no doubt that if chemists would seriously turn their attention to the matter they would be able to get it from the atmosphere. It was sufficient at present to recover that small quantity of ammonia which came off in the earlier stages in the production of coal-gas, or in the earlier stage of heating in the upper part of the Siemens producer. Even in this way an enormous amount of ammonia would be obtained, which would be a source of income to the manufacturer. Mr. Dowson had spoken in glowing terms of the Brin process for making oxygen, which had been shown at the International Inventions Exhibition and described at the meeting of the British Association in Aberdeen. As Dr. Armstrong happened to preside in the section to which the communication was made, he naturally took an interest in the question, and he thought it would be wrong if Mr. Dowson's eulogium were allowed to go forth as it at present stood. No doubt many persons must have been struck with the charming arrangement of the apparatus. The process was the old one, which had been tried from time to time and found wanting, except that instead of merely heating the peroxide of barium formed by passing air over oxide of barium, Mr. Brin reduced the pressure. Practically the gas was extracted under almost no pressure, and that was a very material improvement in the process; but so far as he understood, no method had been introduced which conferred perpetual life upon the materials used to absorb the oxygen. It had been stated that the material absorbed more and more readily the longer it was used, but up to the present time no evidence of that had been given. Dr. Armstrong.

Mr. B. Dawson regretted that the Author had not given a better illustration of the number of different industrial processes to which gas-producers had been applied. It appeared to him that strictly a gas-producer was a process by which gas could be taken any reasonable distance and applied to several processes at the same time. Perhaps the Author would explain the difference between semi-producers, in which the gas left the producer in a state of semi-combustion, and those in which the gas could be taken a considerable distance. Mr. Dawson.

Mr. A. K. Mackinnon thought the general consensus of opinion was that gas-producers were not a very great success; and this was confirmed by the statement of the Author, that it was highly probable that in future efforts would be directed towards the perfection of means of producing gas free from the dilution of the Mr. Mackinnon.

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Mr. Mackinnon. large quantity of nitrogen derived from an air-supply, as that system not only afforded the means of obtaining high temperatures of combustion, but also tended to simplify the furnace-arrangements, rendering it necessary to heat the air only prior to combustion, and not, as at present, both air and gas. Mr. L. de Soulages of Paris three years ago invented a furnace and gas-producer on totally distinct lines to any that had been discovered up to the present moment. His furnace was between a reverberatory- and a blast-furnace. There were two blow-holes for admitting air, and one blow-hole for carbonic oxide. As soon as sufficient heat had been obtained, say, 1,500°, the air-holes were stopped, and carbonic oxide was introduced; the metal, in a pulverized state, was then dropped in, and it began to reduce rapidly; then the carbonic acid was given off, and it was carried away by one of the chimneys, collected, and taken to a furnace, where it was decomposed into carbonic oxide and collected in gas-holders. The process was continuous, and from all that he had heard it was very successful. It had gone beyond a chemical experiment in the laboratory, for at the present moment large works were being carried out in France on that system. It required about 450 cubic feet of carbonic oxide per ton of ore smelted, being 9,000 cubic feet per 20 tons. The chief merit in the system, he thought, was the furnace, where the carbonic acid was decomposed, and where the carbonates were heated in order to expel the carbonic acid. The carbonic acid was allowed to pass over incandescent charcoal or coke, and that took place direct from the furnace. The carbonic acid also, which was generated from the carbonates, was decomposed there and went direct to the gas-holder. The system appeared to be all on proper lines. He would not say that it was perfect, but he had no doubt that the ingenuity of manufacturers would improve upon it. It might be a question whether it would be desirable to have not only the carbonic oxide but another blow-hole with carburetted hydrogen, which could be enriched by passage through various hydrocarbons, so as to make it more effectual.

Mr. Crowe. Mr. Henry Crowe (for Mr. R. Howson) said that the Author had been good enough to refer to the down-draught gas-producer, on which Mr. Howson had been recently experimenting, and it was to be regretted that he had not been in possession of more complete data on which to form a judgment. So far, however, as his remarks went they were perfectly just, but Mr. Howson proposed to supplement them with a few additional particulars. His main object in commencing the trials was to devise a gas-producer which should

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be free from the objectionable features of those now in use. It was a fact, familiar to persons who had experience in such matters, that in all those producers which were not of the closed retort class, there were two special nuisances which entirely excluded them from a great number of applications where cleanliness was an object. One of them was the accumulation of tar and coal-dust in the flues, and the other was the escape of smoke from the upper part of the apparatus. It was in vain to ask the proprietor of a well-appointed factory to have his boiler-shed subject to the chronic nuisance of smoke blacker than that which issued from the chimney-top, and to the periodic firing of what was equivalent to a hundred tar-barrels. Those were the evils to be cured, and he thought that every one who had seen the down-draught producer in action would agree with him that it did cure them effectually. If the apparatus was not quite a fit object for a drawing-room, it might at all events be placed outside the open window of a drawing-room without choking the inmates, while the flues, after being many days at work, appeared to be perfectly free from deposit. So far it would be admitted that he had been able to make out a good case, but it might be asked had his method no defects? He was not disposed to adopt the word defects, but he acknowledged that there had been difficulties to overcome. The conditions of success were obviously different from those of the usual construction. In the first place, combustion did not proceed so freely downwards as upwards, and consequently a stronger blast was required to force the current of gases through the body of fuel. Secondly, the upward-burning producer, being fed at the top while the air entered at the bottom, the fuel, by the time it had reached the tuyeres, had been considerably heated, and so prepared for the rapid attack of the oxygen in the blast. A high temperature of combustion was thus easily attained. On the other hand, when the current passed downwards, the fuel being still fed at the top, the coldness of the latter caused the combustion to be somewhat sluggish, and, when steam was used for injecting the air, there was some difficulty in obtaining sufficient heat for decomposition. It seemed to be absolutely necessary, therefore, either to devise a blower, in which the consumption of steam was small as compared with the air delivered, or to superheat the steam, or to blow air alone into the apparatus. Up to the present time many trials had been made, and the system adopted was shown in Figs. 59 and 60. The current descended through the charge, split on either side of the central bridge, and then passed underneath the bridge into the flue below. The essential requirement was that there should

Mr. Crowe.

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Mr. Crowe. be a more than usually ample supply of air as compared with the quantity of steam used for propulsion. He was inclined to think that a system of blowing with air alone, combined with automatic feeding, would be very advantageous. A catalogue of results which were purely experimental would be out of place. He would only say that a few analyses had been taken, which tended so far to prove that the best effect was accomplished when the air delivered bore the largest proportion to the steam consumed. The following was an analysis taken by Mr. Stead on the 18th of January:—

	Per cent.
Nitrogen	63·50
Carbonic oxide	19·16
Marsh gas	0·34
Carbonic acid	7·50
Hydrogen	9·50
	100·00

The temperature in the flue by a Siemens pyrometer was 2,050° Fahrenheit before combustion. The temperature in the furnace was a welding heat, with air slightly warmed. He made no remarks about the analysis, except that the temperature of the generated gases was extremely high, which was not entirely due to the formation of carbonic acid; also that, if the steam consumed had been less, the percentage of carbonic oxide would have risen in proportion.

Mr. Bamber. Mr. E. F. Bamber thought that the Author had devoted a disproportionate amount of time and space to many of the producers which he had described. Some of them had been applied only to single works; some had hardly been applied at all, and others had been discontinued soon after their introduction. He agreed with the Author that it was due to the late Sir William Siemens that gaseous-fuel had been so much employed; but he was surprised that after that remark he should have spent so much time in pulling to pieces Sir William Siemens' work. Mr. Head had omitted to refer to the Siemens old-type producer with a steam-jet. The Author had condemned that application; but it appeared to have been exceedingly beneficial, especially with some kinds of coal. It had been used both in this country and abroad. Messrs. Krupp, of Essen, had commenced by ordering a couple of steam-jets for use with their producers, and in six months they ordered more than a hundred, showing their estimate of its value with a coal which they had been unable to use, either in a solid-fuel furnace or in a gas-furnace. A valid

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objection to the steam-jet, in that form of gas-producer, was the closed ashpit, and that was also the main objection to the Wilson producer. The reason, of course, was, that if the apparatus was not producing good gas it had to be cut off entirely; but in the case of an open ashpit, it was only necessary to stoke it, and then the gas became good again. With the new type of Siemens gas-producer that objection did not exist. The Author had referred to the difference of temperature produced by burning carbonic oxide, and by burning solid carbon, showing an improvement of about 10 per cent. with the former. There was a certain temperature to which carbonic oxide had to be raised, even assuming that it was pure carbonic oxide, namely, about 1,000° Centigrade, and only the amount from that point up to the assumed temperature of 2,982° was available for heating purposes; whereas, with solid fuel the temperature at which it first ignited was much lower—about 200° or 300°—so that a much larger range of temperature was available. The Author had stated that with an apparatus, which would produce pure carbonic oxide combined with certain hydrocarbons, the furnace-arrangements would be simplified; and under those circumstances it would be necessary to heat the air only prior to combustion, and not both air and gas. He could not understand the reason of that. It appeared to him that if it was useful to heat the air, even when using carbonic oxide and nitrogen, it would be equally useful to do so when using pure carbonic oxide. Mr. Bamber.

Mr. W. H. Thelwall asked the Author whether in applying producer-gas to the heating of boilers, there was any advantage to be derived in the matter of the chimney. He had heard it stated that a much smaller chimney would answer the purpose. Mr. Thelwall.

Mr. C. E. Botley observed that there were one or two practical points in connection with gas-producers which he did not think had been touched upon. Some time ago he was called upon to investigate the use of gas-producers for heating steam-boilers, and he saw a great many different systems at work in various parts of the country; but he could not discover that any great economy in labour was effected by them. The attention was simply transferred from the boiler to the producer where a considerable amount of labour was required, a difficulty very generally experienced, from the fact that the producers required a great deal of attention in keeping them clear. They became caked over the top, necessitating a great amount of labour to get a vent through them. The clinkering was also a great difficulty, false bars having to be inserted. The general opinion seemed to be that though Mr. Botley.

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Mr. Botley. uniformity of heats and cleanliness could be obtained at the point of consumption, there was no saving in labour. It had been proposed to overcome the difficulty in clinkering by Brooke's self-cleaning gas-producer, a description and illustration of which would be found in the Transactions of the Gas Institute for 1885, p. 140. With reference to the utilization of nitrogen, he did not think that would be a great point, especially at the present time, because sulphate of ammonia had been going down in value, realizing only half the amount obtained for it three or four years ago. He thought that the gas profession could fairly claim credit for having pushed the use of gaseous-firing as much as possible, although the fuel was coke. Mr. W. Foulis, M. Inst. C.E., of Glasgow, in particular had done extremely well with the Siemens producer, and many other types had been introduced at other works. The only obstacle to its general adoption appeared to be the expense, especially when used with regenerative chambers; but a modified form of furnace had been introduced into many gas-works, and there was no doubt that the advantages of it were enormous. Uniformity of heats and perfect combustion were secured; the trouble of clinkering was done away with, and great economy was effected in the consumption of fuel. It had been stated that coke was a drug in the market. Whatever it might have been, it was not so at the present time, for it was highly appreciated by the general public, and the desire therefore would be to produce more, instead of less. Liquid-fuel had been introduced in one or two cases with great advantage, and it was a matter engaging deep attention. He might be permitted to direct attention to another use of gas-fuel, which was very much on the increase, namely, the application of ordinary coal-gas by the aid of a blast. It had been tried for many years for taking off the tires of railway carriage-wheels and putting them on, heating rivets, straightening bent axles, and many other purposes, and when the gas was made by those who employed it, the method was economical, clean, and perfectly successful.

Mr. Dawson. Mr. B. Dawson said that a question had been asked with reference to the height of chimneys. According to his experience, he did not think it advisable to reduce their height, but the sectional-area might be diminished.

Mr. Cowper. Mr. C. E. Cowper remarked that a dozen years ago, when he was on the staff of the late Sir William Siemens, he had a good deal of experience with gas-producers. He agreed with Mr. Bamber that the Author had not given sufficient prominence to the original type of producer, which had done far more work than

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had the many modified forms exhibited. Moreover, it was the requirements of the Siemens regenerative furnace that brought the gas-producer into existence; it was a matter of history that the regenerative principle was tried by Mr. Siemens for heating the air for the combustion of solid fuel, and that the difficulties in the adoption of the latter led to the adoption of gas-fuel. The Siemens producer—even the old type—was very largely employed; he had had to do with hundreds of them; they were to be found in many works in this and other countries, and they effected, in conjunction with the regenerative furnace, an enormous saving of fuel. When coal was used, in Sheffield, in producers for melting steel, 1¼ ton of common coal was the charge, instead of 2½ to 3 tons of good coke. With regard to boilers, there had never been any great economy; the use of gas, however, was very convenient, because it kept all the coal and dirt outside the works. It was unreasonable to expect great economy, because with a well-arranged Cornish or Lancashire boiler the whole of the heat that could be developed by the coal was utilized, as far as it was practically available; with gas-fuel more could not be expected. But with furnaces for re-heating or melting—furnaces in which a very much higher temperature was wanted—greater heat could be obtained, because the gas was used with the regenerators in which the heat accumulated, and developed a temperature not wanted for boilers. Mr. Cowper.

Mr. Radcliffe Ward said it appeared to him that the main point to be considered was whether the appliances described could be introduced with economy into every-day engineering work. Some months ago he inquired into the working of a proposed electric installation; the Wilson gas-producers were estimated for, and the result which the makers were prepared to guarantee gave a net saving on the fuel consumed of at least 25 per cent., excluding the amount of gross saving required to nullify the extra capital needed for the producers. He thought that this practical result was worth a great deal of theorizing as to the saving that might be effected in the by-products. In the case in question by-products were ignored altogether. Evidently there could be a saving, because the combustion of coal per lb. was more complete, and there was less waste of the products of combustion up the chimney. In the next place, the coal was considerably less in price per ton. It appeared, therefore, that the appliance was a remarkable one, capable of introducing economy into every-day engineering work, and was especially of interest in connection with electric-lighting work, where economy of fuel Mr. Ward.

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Mr. Ward. was of great importance. In the estimate to which he had referred, the saving was sufficient to make a difference of 1 per cent. upon the proposed dividend which the installation would have paid.

Mr. Rowan. Mr. ROWAN, in reply, said he had no reason to complain of the tone of the discussion in general. A good deal of interesting information had been added to that of the Paper, and the Proceedings of the Institution would be enriched thereby. He desired, however, to state, in order to clear away a good deal of misunderstanding, that the object of the Paper was not to discuss the advisability of using gaseous-fuel as compared with solid fuel, nor did he propose to enter into the question of different methods of using gaseous-fuel. His object was entirely to deal with various designs of separate producers. Some forms of furnaces had been mentioned, especially by Mr. Bauerman, which had fireplaces whose action was similar to that of separate gas-producers; but they introduced another field of discussion. Mr. Dawson had referred to several of the producers illustrated as being of a somewhat similar character; but he did not understand to which of them Mr. Dawson alluded. His aim had been to exclude all but those which might be considered as separate gas-generators, which by the decomposition of the coal produced gas that might be conducted through a greater or less distance to the furnaces in which it was to be consumed. Mr. Head had inferred that the Paper was on the Wilson gas-producer, and other speakers had stated that an unfair amount of attention had been devoted to criticisms on the designs of Sir William Siemens. From those remarks he emphatically dissented. The illustrations were witnesses against the statement of Mr. Head, although it appeared from the remarks he had made, as if the names attached to the illustrations ought not to have been there, or that they were there only by permission of Messrs. Siemens. He had endeavoured to be impartial in dealing with the various designs. His object had been to show the various plans proposed for constructing and working separate gas-producers, in order that they might be compared one with the other. He had taken it for granted that the question of the advantage of using gas as against solid fuel was settled, and in the main he believed that it had been settled; it was not necessary, therefore, to enter into a discussion of that question. He had another object in view, that of showing the direction in which improvement in methods of producing gas might be expected to proceed, namely, in that of producing gas free from diluent nitrogen, and his view had been corroborated by many

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of the remarks made in the course of the discussion. The reason why advantages were to be derived from gas of the description to which he had referred might be found in the fact, that at present a large mass of inert nitrogen required to be heated, so that the temperature of combustion might not be unduly lowered. If that nitrogen were eliminated it would not be necessary to heat the gas and air to the same point as at present, apart altogether from the point raised by the remarks of Dr. Armstrong as to the higher radiation to be expected from the combustion of gas of such a quality. Mr. Bauerman's reference to the blast-furnace as a gas-producer was excellent, and showed how the producers which were worked rapidly were likely to be, as had been found to be the case, more economical than those worked more slowly. With reference to the production of gas free from nitrogen, Mr. Head, he thought, had misapprehended the intention with which he had introduced Mr. Galloway's equations, and that had led Mr. Head to state that the equations had been misapplied. They were undoubtedly evidence of the possibility of producing higher temperatures with gaseous-fuel than with solid fuel; but the conclusion to be drawn from them was certainly not the one drawn by Mr. Head, but the one referred to in the Paper, namely, that gas nearly approaching pure carbonic oxide should be produced, because the existing producers did not furnish a quality of gas fairly comparable with the pure carbonic oxide mentioned by Mr. Galloway. He could not admit that the cooling-tube was the best form of cooler. Cooling was no doubt necessary in certain cases, such as those mentioned by Mr. Head; but apart from these it ought not to be necessary, because a producer making gas which carried undecomposed steam was not making good gas —gas from which the highest temperature of combustion might be obtained. As to his remarks about the use of the cooling-tube in the modified Siemens old-type producer, he had stated a fact upon the authority of some results given by Mr. Head himself in a Paper read before the Iron and Steel Institute.[1] He had shown in that Paper, that whereas gas from the original Siemens producer was reduced in bulk by cooling to atmospheric temperature from 120 cubic centimetres to about 85 (a reduction of 35 cubic centimetres); gas which was subjected to a process resembling that in the new producer was only reduced 20 cubic centimetres; so that the action of the cooling-tube under such circumstances could not be so complete, and therefore the working of the producer

Mr. Rowan.

[1] The Journal of the Iron and Steel Institute, 1885, p. 126.

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Mr. Rowan. could not be so good. But as against that the use of an exhauster had been introduced, and Mr. Head counted a good deal upon the regularity of the effect of the exhauster. It was, of course, possible to make an exhauster which worked regularly, passing regularly a certain volume of gases; but that did not meet the difficulty, because it was well known that the production of gas was not a constant effect; it depended upon the state of the fire and the periods of charging. At certain periods, a much larger volume of gas was produced in a given time than at others. It would therefore be better if the exhauster did not work regularly. He should be sorry to be thought guilty of having tried to pull to pieces any of the work of the late Sir William Siemens. In explanation of the remark which he had made, he might be permitted to say that he had seen the circular producer worked by chimney-draught at the Forth Bridge Works.

Mr. Head. Mr. JOHN HEAD objected that he was not responsible for the manner in which those producers were worked, for although Sir William Siemens furnished to Messrs. Tancred and Arrol the drawings of a heating-furnace and gas-producer, that was done upon some understanding of which there was no record at his office; and that furnace was set to work without Sir William Siemens' or Mr. F. Siemens' assistance or directions having been sought. The operations carried out at the Forth Bridge could not therefore be cited as an example of the usual practice in working the Siemens gas-producers.

Mr. Rowan. Mr. ROWAN said it was moreover manifestly impossible to obtain what might properly be called a pressure of gas in any producer which had a bottom open to the atmosphere, and he had, therefore, made the remark which had been objected to in the Paper. He need not say that he had the greatest respect for the work of Sir William Siemens, but he did not think that a wholesale claiming of the plans which had been introduced by others, added any lustre to an undoubtedly great name, and he was sorry to find Mr. Head appropriating some of the various designs that had been mentioned, by saying that they had all been anticipated in the patents of Messrs. Siemens. He had carefully looked at the designs described in these patents, and he failed to find any resemblance between them and those in the illustrations, with one exception, that a conical, tapered upper portion of the producer, which was called a retort, was shown as applied to the sloping-grate producer. That idea had been worked out by others from Ebelmen onwards in different forms, and it certainly did not constitute the designs in question copies of Messrs. Siemens' patent.

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Mr. Rowan.

He had not claimed (as Mr. Dowson appeared to think) for Mr. Wilson the first introduction of the steam-jet; but only its first application to a producer with a closed hearth. A closed ashpit was, he believed, allowed by Mr. Bamber to be an objection to the application of the steam-jet, especially where, as was the case with producers having grate-bars, a great thickness of fuel could not be used. In the case of the Wilson producer, that of Tessié du Motay, and of one or two others, no ashpit, in the proper sense of the word, was used, there being no grate-bars. A closed hearth was different, and introduced very different conditions; therefore the use of the steam-jet with producers having a closed hearth was not open to the same objection. On this point he could not admit the correctness of Mr. Head's description of the difference, either in design or working, between the Wilson and the Siemens producer. The use of blast in working producers was not the invention of Messrs. Siemens. It had been used by Ébelmen, Ekman and Beaufumé at least, before their day. Messrs. Siemens applied one form of apparatus for producing blast to a sloping-grate producer, while Mr. Wilson applied another form of apparatus to a producer having no grate, but a hearth like a blast-furnace. The objection to the use of blast in working producers where the blast was properly applied existed only in imagination. Closed-hearth producers could be, and were in great numbers, worked and cleaned without stoppage, and they were quite as amenable to the action of the stoking bars as any producer having grate bars or their equivalent, without the disadvantage inherent to them of being unable to use dross or small coal, and of losing a considerable quantity of fuel by its dropping between the bars during stoking or cleaning. Mr. Head's concluding remark was therefore unfounded. Some questions asked in the course of the discussion had already been replied to, and it remained for him to notice Mr. E. A. Cowper's remarks. As to the decomposition of steam in the Strong producers referred to by Mr. Cowper, he believed, though he could not give the reference, that the fact that surfaces of brick heated to a high temperature did decompose steam, had been stated by Dr. Percy. But he thought it did not matter very much what the surface was. If steam was exposed to a certain temperature, that was sufficient to cause dissociation to come into play. He made that remark with all deference to Dr. Armstrong, who, as he understood from his remarks in this discussion, was not a believer in dissociation. With reference to the recovery of ammonia being incompatible with the production of heating-gas, it was true that if the

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Mr. Rowan. maximum quantity of ammonia were recovered from the coal, the temperature of combustion in the producer was not in that event suitable to the production of good heating-gas, and the apparatus introduced by Mr. Beilby, at Oak-Bank Works, was a case in point. He extracted the maximum quantity of ammonia from the coal, and consequently did not produce a quality of heating-gas which was particularly good, as the analyses he had quoted showed; but at the same time, where the fuel containing nitrogen was burnt in an atmosphere of steam, a very fair quantity of ammonia might be recovered, consistently with the production of good heating-gas.

Correspondence.

Mr. Beilby. Mr. G. BEILBY observed that not the least interesting part of the Paper was the table of analyses of producer-gases; it would, however, add much to its value for purposes of comparison if the numbers representing the calorific value per 100 litres were extended for all of the gases analysed. The want of this extension had led the Author into an error which he would no doubt be the first to recognize, when it was pointed out. On page 5 of the Paper it was stated that the conditions under which Young and Beilby's apparatus was worked were unfavourable for the production of good heating-gas, owing to the considerable quantity of carbonic acid which was formed. The latter part of the statement was true, but it was equally true that a proportionally large quantity of hydrogen was also produced, so that the calorific value of the gas was above, not below, the average of producer-gases. Selecting analyses 15, 26 and 47 as good representative examples of Siemens, Wilson, and Young and Beilby gas (pp. 50, 51), and extending the calculations of calorific value, the following results were obtained:—

	Siemens (No. 15).	Wilson (No. 26).	Young and Beilby (No. 47).
Quality of coal . .	Not stated.	Yorkshire slack.	Lanarkshire slack.
Calories per 100 litres	116,753	124,302	147,433

Evidently No. 47 was susceptible of great enrichment by the removal of carbonic acid (say for chemical purposes), or by its reduction to carbonic oxide by passing the scrubbed gas through red-hot coke.

Mr. Clerk. Mr. DUGALD CLERK remarked that the Paper clearly described the practical points involved in the construction of an efficient producer, as well as some parts of the theory of the use of gaseous instead of solid fuel. He thoroughly agreed with the Author in thinking it likely that gas-firing would in time become much more extended than at present, especially for metallurgical work

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where high temperature was the principal consideration. When high temperature was not the main point, as in steam-raising, he did not believe that the preliminary gasification of the fuel offered much advantage; this, he thought, was apparent from a consideration of the necessary loss in all existing methods of gasifying fuel. With pure carbon in a producer, for example, and air alone without steam, a perfect producer would give a mixture of carbonic oxide and nitrogen only, the composition being 1 volume of carbonic oxide and 2 volumes of nitrogen. The gases leaving would be at a high temperature, and if cooled before reaching the place where heat was to be produced, only 70 per cent. of the amount of heat which the original carbon would give if burned direct would be evolved. That was taking the quantity of heat evolved by a given weight of carbon when burned to carbonic acid as 100, then when burned to carbonic oxide 30 would be evolved, leaving 70 to be evolved by burning the carbonic oxide to carbonic acid. In gasifying carbon there was therefore a loss of heat which could be avoided in either of two ways:—(1) By retaining the heat in the gas formed; (2) by abstracting the heat from the gas formed in such a manner that it might be rendered available in a chemical reaction to give more inflammable gas. The first system was used in some kinds of the gas-firing of ordinary coal-gas retorts, where the gases from the producer impinged almost directly upon the retort without a long passage and cooling. The second method was applied when some kind of regenerator was used to cool the issuing gas and heat the entering air, which in this case must have steam along with it. When steam acted on hot carbon the reaction, as far as it ultimately resulted in producing inflammable gas, was

Mr. Clerk.

$$H_2O + C = H_2 + CO.$$

This reaction could not proceed without a large absorption of heat, for 18 parts by weight of steam and 12 parts by weight of carbon required an expenditure of 39,540 heat-units to produce by decomposition 2 parts by weight of hydrogen, and 28 parts of carbonic oxide. Mixing steam and, in the proper proportions, air supposed to have given to it by regeneration all the heat of the issuing gases, and supposing no loss by conduction, then pure carbon would yield a gas of the following composition:—

	Volumes.
CO =	38·7
H =	16·4
N =	44·9
	100·0

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Mr. Clerk. This gas would on burning give the same amount of heat as the direct burning of the original carbon; it was the best gas capable of production from air, steam and carbon, and might be used as a standard of efficiency of producers. In practice, naturally some loss was necessarily made. None of the analyses on p. 50 showed an efficiency so high as 80 per cent., although several closely approach that figure. A more careful application of the regenerative system was necessary if improved results were to be obtained; but 80 per cent. was a high efficiency, and if practical difficulties were more thoroughly overcome, greatly extended usefulness would follow without the necessity of improvement on this point. For gas-engine purposes the principal difficulty at present was tar; in the Dowson producer this trouble was overcome by burning anthracite, but in order to be generally applicable, ordinary fuel of the cheapest kind would require to be gasified without producing tar. The Wilson producer, excellent as it was for furnace-purposes, was as yet inapplicable to the gas-engine; an engine of his own, tried with it several years ago, could not be worked continuously on this account. The late Sir William Siemens, long as he worked with the gas-engine, and originating as he did the gas-producer, was unable to get the two to work together. The application of the gas-producer to the gas-engine would undoubtedly greatly extend the usefulness of producers and engines alike. The main advantage of gaseous-fuel lay in its capability of application at the point where heat was required, with just the air necessary for combustion and no more, and the ease of using regenerative methods in handling it.

Mr. Coleman. Mr. J. J. COLEMAN observed in reference to the formula given by the Author at the commencement of the Paper, p. 4, that the conversion of the solid fuel into the gaseous was caused in the Siemens, the Wilson, and other similar producers, by the admission of air, the oxygen going to form CO, and carrying along with it the usual quantity of inert nitrogen. Instead of pure carbonic oxide, a gaseous fuel was produced largely diluted with nitrogen; but when it was burned in air, instead of the resulting temperature being

$$\frac{2{,}403}{1{\cdot}57 \times 0{\cdot}2164 \times 1{\cdot}91 \times 0{\cdot}244} = 2{,}982^\circ \text{ Centigrade},$$

it was much lower, namely,

$$\frac{7 \times 2{,}403}{11 \times 0{\cdot}2164 \times 26{\cdot}8 \times 0{\cdot}244} = 1{,}805^\circ \text{ Centigrade}.$$

The temperature of combustion of carbon, 2,717° Centigrade, was

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therefore 912° Centigrade higher than that of the CO obtained in the ordinary gas-producer—this difference of temperature appearing as sensible heat in the gaseous products, and in the case of gas from coal a portion of it disappearing as the latent heat of the distillation of the hydrocarbons. This calculation was, of course, somewhat modified by the fact that the small quantity of steam used in ordinary producers (1 part of steam to 20 parts of air in the Wilson producer,) formed water-gas, to which the formula quoted by the Author applied. As water-gas existed only in small quantities in producer-gas, and as the Author subsequently stated "that a continuous water-gas producer has still to be invented," it was evident that the formula adduced had little bearing on the various processes described. In the Wilson producer, the Author stated that "this upper portion should be a veritable retort." No doubt this was as it should be; but it would be evident, to any one experienced in the slowness of the distillation of coal as carried on in the ordinary gas-retorts, that this upper part of the Wilson furnace was far from being so, taking into consideration the thickness of the walls of the conical part, and the shortness of the chamber containing the fresh fuel, as well as the comparatively low temperature of the gaseous-products. The Minary producer, with its retort of iron, would be much more efficient in this respect, and both retorts, he thought, would fall short of the Gröbe producer, which had the two operations entirely distinct. He had no doubt that the regularity of the gaseous-products claimed for the Wilson producer was due principally to keeping the fuel as high in the retort part as was practicable, always ensuring that the incandescent coke covered the outlets for the gas, and also taking care to have a fresh charge put in before the hydrocarbons had been entirely distilled from the last one. In the Wilson automatic producer (Figs. 43, 44, 45) the retort part was entirely dispensed with, an arrangement which must of necessity give a supply of gas very irregular in quality; such a producer could not be worked to advantage singly. An automatic supply of fuel under such conditions would certainly be a much greater desideratum than a regular discharge of the ash, which with ordinary coal would not exceed 10 per cent. of the fuel used. Fig. 73 (p. 82) represented a producer which had been introduced successfully by Mr. J. W. Macfarlane, of Glasgow, and which had some excellent features, more especially the ease with which it could be cleaned out, and in the regularity of the gaseous-products. The first of this kind was erected in the Lochburn Iron-works, about seven years ago, and had been regularly at work

Mr. Coleman.

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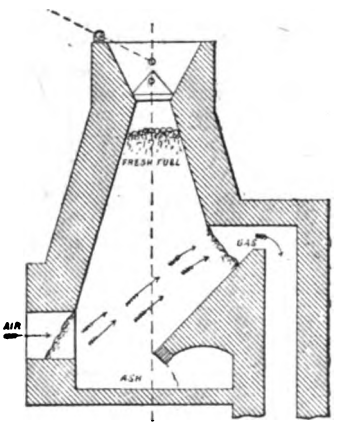
Mr. Coleman. since that time, drying cast-iron pipe-moulds. Quite recently one of these producers had been erected in a large engineering works in Glasgow, the gas from which had been used successfully for all the drying and melting required in a large brass-foundry; that was to say, it had been used in a large reverberating furnace, provided with a Gorman continuous regenerator for heating the air for combustion; and it had also been used for melting in crucibles placed in specially constructed furnaces designed by Mr. Macfarlane, which were so arranged that the crucible formed an integral part of the furnace, and was capable of being tilted without being removed from the furnace, and consequently never needed to be touched with the tongs, as usual in ordinary brass-founding. Any one examining the various operations could not but be struck with the

FIG. 73.



MACFARLANE PRODUCER.

ease and cleanliness with which they were performed, and with the apparent regular supply of gas from one producer, and the facility with which the flame could be regulated by the air-supply. In this producer the air, which was supplied by a Korting blower, entered the natural slope of the ash, passed diagonally upwards through the incandescent coke, and left by the natural slope of the coke. The ash mostly accumulated where the air entered, and was easily and rapidly withdrawn through the doors. The bell in the hopper was made to open upwards instead of downwards as usual, which was intended to allow of its being changed without stopping the producer entirely. The column of fresh fuel rested on the bed of incandescent coke, and the hydrocarbons were

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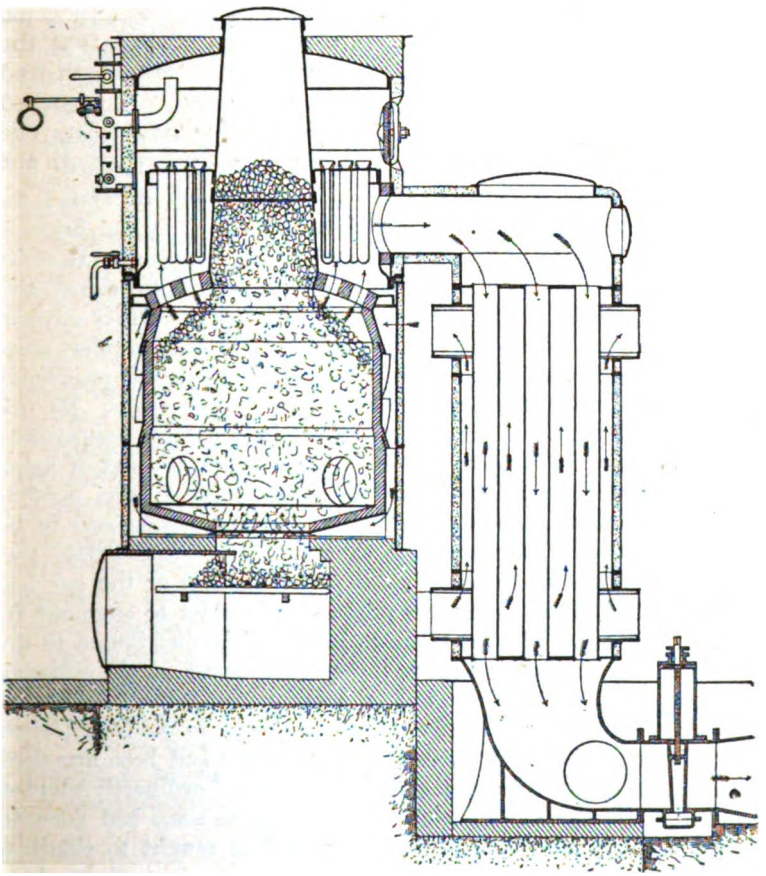
gradually distilled, and had to pass through a portion of the incandescent mass before finally passing into the outlet flue. Mr. Coleman.

Mr. A. Fichet submitted a description of Muller and Fichet's Universal Gas-producer. The apparatus (Fig. 74), which was designed in June 1874, consisted of a cylindrical or rectangular Mr. Fichet.

Fig. 74.



body of boiler-plate, or cast-iron, furnished with a lining of refractory material. In this vessel the fuel was gasified. Air was forced in at the lower part by means of a steam-jet, and the slightly funnel-shaped base acted to direct the mixture of steam and air to the middle of the fuel. The top of the retort was formed by a firebrick arch perforated by holes, allowing the

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Mr. Fichet. gas to pass to an upper chamber. Through the axis of this chamber passed a hopper of boiler-plate, resembling a truncated cone, attached to the roof of the retort, and serving for the introduction of the fuel. The latter descended little by little till it reached the retort, where it was distilled by the heat of the gases passing through the perforated arched roof. The heat from this source also served to generate the steam necessary for admitting the air. To this end the upper chamber formed a ⌐¬-shaped steam-boiler furnished with Field tubes, distributed around the hopper which passed through it axially. This disposition utilized the greater part of the heat of the gases of combustion without any admixture of air. In order to cool them completely before distribution, they were, on their exit, made to traverse, in a downward direction, a nest of tubes around which circulated the mixture of air and steam furnished to the retort by the injector. By this means the air was superheated without expense. In order to avoid loss of heat by radiation, the generator was furnished with a double envelope, the outer wall of which was further covered with a layer of non-conducting material. In the annular space thus provided the air to feed the furnace circulated, previously heated in its passage round the tubes of the gas-cooler. By this means the mixture of air and steam attained a high temperature before coming in contact with the fuel, and thus allowed of using a higher proportion of steam than would have been possible without such superheating. The steam was decomposed in its passage through the generator, thus providing combustible gas. It might therefore be said that the sensible heat of the gas had been utilized at the time of its production in order to augment its calorific power. The gas being cooled, could be distributed to the various furnaces of a factory, to be there burnt in the ordinary way with cold or hot blast, and on occasion could itself be heated by the passage through regenerators, according to the heat desired to be attained in the furnaces. This generator had been arranged so as to utilize inferior coal, and such as was not ordinarily adapted to the gas-producer. Instead of attempting, as some had done, to melt the ash, Messrs. Muller and Fichet had sought to obtain a moderate rate of working. A convenient number of openings at the bottom of the generator, allowed of its being cleaned out. Generally the burning fuel reposed on a bed of ash and scoria easily permeable by the air. When this bed became too thick some of it was cleared out. In ordinary work it sufficed to do this once a day. The joints were made with asbestos cloth, which prevented the escape of the gases. Below the tubular

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cooler was a casting of one, two, or three pipes, furnished with valves to direct the gas into the required direction. With such a disposition of apparatus it was possible to install a gas-producer in some convenient corner of the factory, and to conduct the gas from it to any required point as was done with illuminating-gas. To obviate an inconvenience arising from irregular consumption, a gasholder might be employed, thus ensuring a supply at constant pressure, and rendering the working of the producer and of the furnaces mutually independent. As regarded rate of working, this producer would easily gasify 100 to 120 kilograms of fuel per square metre of section (20·5 to 24·6 lbs. per square foot) per hour, and by slightly augmenting the force of the blast the product could be increased. When coal of too rich a quality was employed, distillation was produced in the hopper surmounting the retort, and this caused the accumulation of a mass of coke which would not feed properly into the vessel below. If such fuel were used it would be necessary to leave at the base of the arched roof several holes, through which iron bars could be introduced to break up the coke. In conclusion Mr. Fichet justified the name "Universal Gas-producer" by the consideration that the apparatus, being altogether separate and distinct from the furnaces in which the gas was to be burnt, was capable, either singly or by grouping, of making the gas for all the furnaces of a factory, and was applicable to nearly all industries. Among the fuels of little value which could be utilized with advantage in this producer were coke-breeze, often a waste product, and the cinders falling from the grates of ordinary furnaces. Such materials could be charged in the hopper either alone or mixed with coal, and would furnish excellent gas. Mr. Fichet had the pleasure to present to the library several pamphlets bearing on the employment of producer-gas.[1]

Mr. Fichet.

Mr. W. Foulis stated that about four years ago, shortly after Sir William Siemens patented the circular form of gas-producer referred to by the Author, he proposed to Mr. Foulis to apply a producer, somewhat similar in construction, to heating gas-retorts.

Mr. Foulis.

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[1] "Notice sur les travaux scientifiques de M. Ébelmen." Tract 4to. Paris n.d.
"Notice sur l'appareil de M. Orsat pour l'Analyse des Gaz." Par M. Fichet. Tract 8vo. Paris, 1873. (Extrait des Mémoires de la Société des Ingénieurs-civils.)
"Etudes sur la combustion et sur la construction rationnelle des Foyers industriels." Par A. Fichet. Tract 8vo. Paris, 1874. (Extrait des Mémoires de la Société des Ingénieurs-civils.)
"Exposition Universelle de 1878. Notice sur les appareils présentés par MM. Muller et Fichet, Ingénieurs, &c." Tract 8vo. Paris, 1878.

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Mr. Foulis. As in this case coke was the fuel to be used, all arrangements for collecting the gas and drawing it through a portion of the fuel were unnecessary. Four producers somewhat resembling in shape that shown by Fig. 54 were erected at Glasgow Gas-works. This form of producer was soon found to be unsuitable for the fuel then in use, which consisted entirely of the coke from Cannel coal having from 15 to 25 per cent. of ash. Many modifications in shape were tried, and it was ultimately found that the most suitable form was a plain cylinder of equal diameter throughout, without grate-bars, and having four openings for air at the bottom, sufficiently large to allow a shovel being inserted to remove the ashes. These openings had no doors, but were left always free. There was a jet of steam at each opening, the object of which was to prevent the ash from clinkering. Various arrangements were tried for getting the necessary steam from the evaporation of water by the heat of the producer, but in all these arrangements the quantity of steam produced was more or less variable and uncertain: and as the amount required was very small, provided the supply was constant, it was decided to take a supply of steam from the steam-boilers of the works. The best results were obtained when the steam was made to play softly over the whole opening, so as to mix thoroughly with the air passing in to support combustion. These producers had proved highly successful, and there were now in Glasgow Gas-works more than two hundred of them, heating about seventeen hundred retorts. The producers were charged every six hours with red-hot coke drawn directly from the retorts. The whole mass of fuel might therefore be considered to be in a state of combustion from the time the producer was filled; and there was therefore a fairly equal production of gas during the charge. Many experiments had been made on the composition of the gas at different periods of the charge. The following analyses might be taken as an average of the quality of the gas at different periods:—

	One hour after filling Producer.	Three hours and thirty minutes after filling.	Five hours and forty minutes after filling.
Combustible gases—			
Carbonic oxide and hydrogen .	32·7	34·7	37·6
Non-combustible gases—			
Carbonic acid	7·2	5·3	3·4
Nitrogen	60·1	60·0	59·0
	100·0	100·0	100·0

It would be observed that the gas improved in quality as the charge burned down, that was when the depth of fuel was less

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and the temperature was higher. All the experience with these producers went to prove that in order to get good gas it was necessary to have a high temperature in the producers. The successful use of gas for heating depended much on the method of using it in the furnace. The arrangement of ports for the admission of air and gas had to be varied according to the purpose for which the heat was required. When a very high temperature was desired over a limited surface, as for example in the case of welding iron, the air and gas should be made to mix thoroughly immediately on entering the furnace. When, on the other hand, a more moderate temperature, extending over a larger surface, was wanted, as in heating gas-retorts, this immediate mixing should be avoided, and the air and gas should be made to flow through the furnace side by side, gradually mixing as they advanced. Much of the success of the furnaces at Glasgow was due to the careful arranging of the gas- and air-ports for this purpose, so as to get an equal temperature throughout the whole of the retorts without intense local heat at any point. As an example of the benefit of gaseous-fuel, he might mention that since the introduction of these furnaces only one-half the fuel was used, while the retorts were capable of carbonizing 30 per cent. more coal. Mr. Foulis.

Mr. B. D. HEALEY remarked, on the building of curtain-walls for the purpose of compelling the hydrocarbons to pass through the incandescent fuel, that in 1864 he applied this plan to sixteen producers at the Barrow Steelworks, from drawings supplied by the late Sir William Siemens. They did not answer well, owing to the charging hoppers remaining in their former and accustomed position, namely, at the top of the incline in front of the grates, the reason being that the fuel was too thin over that part of the grate below the arches, and the gases generally passed, not through the incandescent part, but through the layer immediately above, which was not hot enough to effect the fixing of the hydrocarbons. He then removed the charging hoppers to a central position over the fire-grates, which enabled him to keep a layer of fuel nearly uniform in thickness over the whole of the grate surface, and to fill up some distance above the arch, without having too much fuel on the incline. Under these conditions, the quantity of soot and tar in the tubes was considerably reduced; but as the producers came in turn to be repaired, the curtain-walls were not properly attended to, occasionally a ring of the arch would fall, and in course of time they were abandoned. Since then automatic feeders (Dowker and Smith's) had been substituted for the central charging hoppers, and these feeders he considered were of great Mr. Healey.

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Mr. Healey. assistance in securing a regular quality, as well as a more constant supply of gas, and the soot and tar were also thereby reduced. In building producers at the same level as the furnaces they had to supply, it was of course optional whether steam-blowers or siphon-tubes were used, but he had always insisted upon keeping the producers below the furnaces where practicable. It was a great advantage to have a decided pressure at all times. During clinkering, when the pressure depended upon blowers, the back-pressure at the fires which were being clinkered was very inconvenient to the workmen. Moreover he had repeatedly heard of explosions being caused in small installations where only two or three producers were used, which would not have occurred had the level of the grates been below the level of the furnaces. At the present time he was using his own producers (with steam-blowers), a modification of the form for which he obtained a patent in 1872. They were liked by the workmen, being handy to clinker, and there was very little loss of coke in clinkering. He had also fitted to the eight water-gas producers at the Trimsarau works annular blast-boxes, with cleaning holes opposite each jet, and with elbow-pipes fitted with doors, under which was a tramway for taking away the clinkers.

Mr. Isherwood. Mr. B. F. Isherwood remarked that there were many circumstances in which the production and use of gaseous-fuel might be advantageous, irrespective of economy. For example, the houses of a city might be supplied with such fuel as a matter of convenience without regard to cost, just as illuminating-gas was generally used, although the same quantity of light could be produced at far less expense by burning the various distillates of petroleum in suitable lamps. And many other cases might be cited, in which advantages other than economy decided the use of particular articles of consumption. The various gas-producers which had been invented, and the results obtained from them, were well worth study and experimental investigation, because their use might be desirable for other reasons than that of a cheaper production of heat, which appeared to be the principal claim at present advanced for them. Nevertheless, the belief that a given quantity of heat could be produced by such means, at less cost than by burning coal in a solid state on a grate in the usual manner, must be dismissed as erroneous, and the benefits to be derived from gas-producers must be considered from a different aspect than an imagined superiority of the combustion of gaseous-fuel over that of solid fuel. The assumption was made as a matter of course, that the consumption of coal on a grate, as in the furnace

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of a steam-boiler, for example, was imperfect, and that consequently a considerable portion of the possible heat obtainable from the coal was lost. In other words, there was taken for granted that the combustible constituents of the coal were not burned to saturation with oxygen, that a large portion of the fixed carbon was converted into carbon monoxide instead of into carbon deutoxide, and that a large portion of the volatile matter wholly escaped combustion. Now in these respects the exact facts relative to the ordinary combustion of coal admitted of being known with absolute certainty by chemical analysis of the gases of combustion drawn from the chimneys of steam-boilers. The mean of many hundreds of such analyses from different chimneys, and for different bituminous coals consumed, showed that a given bulk of these gases, exclusive of aqueous vapour, contained about 0·038 per cent. of carbon monoxide and about 0·025 per cent. of free hydrogen: no hydrocarbons were found, but from 9 to 9·5 per cent. of free oxygen was present, proving that double the quantity of air passed through the fuel that was chemically used for the combustion. The above percentages were for very bituminous coals producing much black smoke, and represented a loss of about one-twelfth of the coal thrown into the furnace; which twelfth, or about 8·3 per cent., passed into the atmosphere unconsumed. In the case of anthracite, about one-fortieth of the quantity thrown into the furnace, or about 2·5 per cent. of the coal, escaped into the atmosphere unconsumed, and the free oxygen found in the dry gases of its combustion was about 10 per cent. of their bulk, showing that twice the air used in the combustion passed through the anthracite. Between these two extremes of solid fuel, the very bituminous coal and the anthracite, the loss varied from the 2·5 to the 8·3 per cent., according as the proportion of fixed carbon was greater or less. To these narrow limits then was restricted any possible improvement in the economic combustion of coal; and as absolute perfection could not be commanded by any method, only a portion of this small margin could be saved. In the foregoing statement of the results of the analyses of the gases of combustion was found the fact that twice as much air passed through the coal as was chemically needed for its perfect combustion; and this proportion obtained for all kinds of coal, from the most bituminous to the purest anthracite. The additional quantity of air, which passed through the furnace unaltered, of course involved a loss of heat represented by the product of its weight, specific heat, and number of degrees between its temperature when entering the furnace and when leaving the

Mr. Isherwood.

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Mr. Isherwood. boiler. This was a very large loss under the most favourable conditions; but it could not be lessened by any previous conversion of the coal into combustible gases and then burning them in the air, which was the method of the gas-producers, although the assumption—and it was only an assumption—was made as another matter of course, that the excess of air was due to the fact of the coal being in lumps, and that consequently much air flowed through the interstices without giving up its oxygen; but that if the coal were previously reduced to combustible gases and then mixed with the air, no more of the latter would be required than was chemically necessary for the combustion. The fact was that hydrocarbon combustion took place between only certain limits of temperature, the lowest being that of ignition, and the highest being that of the dissociation of the compound gases of combustion. Between these lay the temperature of maximum effect; that was to say, the temperature at which the mutual attraction of the hydrocarbon elements and of the atmospheric oxygen was a maximum. As the temperature produced by combustion increased, this attraction decreased, and only a portion of the oxygen would combine, the remainder continuing in the free state. When the temperature of the furnace, or more precisely, that of the gases of combustion at the moment of their formation, attained about 2,000° Fahrenheit, which was the temperature found in practice, exactly one-half of the atmospheric oxygen entered into combination, the remaining half continuing free; consequently, to consume the whole of the coal, twice the quantity of air necessary to furnish the oxygen chemically needed had to pass through the furnace. These facts, discovered by Bunsen, were proved by the every-day results of combustion in the furnaces of steam-boilers, and by the analyses of their gases of combustion. Such being the case, exactly the same double quantity of air chemically needed would be required for the complete combustion of the hydrocarbons, whether in the solid or in the gaseous state. And, further, the temperature of the gases of combustion, at the moment of their formation, would be exactly the same whether the hydrocarbons were burned to oxygen saturation in the solid or in the gaseous state. With a free supply of air, as in the case of the furnace of a steam-boiler, pure carbon would burn completely to oxygen saturation, and there would be no loss due to incomplete combustion. The reason why the hydrocarbon compounds were not completely burned was because their temperature of volatilization was lower than their temperature of ignition, and the rapidity of their passage from the furnace to the chimney-top was so great, that more

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or less of them escaped into the atmosphere before they could attain the latter temperature. The loss of fuel, due to incomplete combustion, was wholly of the volatile hydrocarbons; the fixed carbon always burned completely to carbon deutoxide. Of course, if a very thick mass of coal were placed on a grate whose area of air openings was very small compared with the mass and depth of the coal, the fixed carbon could be largely burned to carbon monoxide, and the volatile portion could be nearly wholly distilled unburned, as in the case of gas-producers; but when the coal was properly burned with such a supply of air as was commanded on ordinary grates, and with the depth of coal employed in the furnaces of steam-boilers, with the view of obtaining the maximum heating effect, there was not the slightest difficulty in obtaining the results herein previously stated. Various attempts had been made to improve the assumed very incomplete combustion of coal on ordinary grates; one of the oldest methods was the "dead-plate" or coking-plate invented by Watt, and placed at the front of the furnace. The fresh coal was laid upon this plate and coked by the heat of the fire beyond it, the volatilizable portions passing over the incandescent fuel in their way to the chimney, and being thus subjected to a sufficiently high temperature and with sufficient air present, the assumption was that they would be completely burned. This system had been largely imitated by the step-grate in continental Europe. The whole idea was based on fallacious assumptions, and careful experiments had proved that nothing whatever was gained by its use with even the most bituminous coals. Neither had anything been gained by finely pulverizing the coal, mixing the dust with air, and blowing the mixture into a furnace, on the grate of which was a mass of incandescent fuel, with a view to the more intimate mixture of the coal and air. Equally complete had been the failure of the Beaufumé gas-producer. In this system the fixed carbon of the coal was burned in one vessel or gas-producer to carbon monoxide, distilling over with it the volatile hydrocarbons which, together with the carbon monoxide, were burned as completely as possible in the furnace of another vessel called the boiler, the heat being used to vaporize water. The gas-producer was surrounded by water which was pumped into the boiler, so that no heat, except by external radiation, was lost in the producer. The combustion in the boiler was wholly of gaseous-fuel mixed with air; nevertheless the most accurate experiments made with this system in the French marine arsenal at Cherbourg showed that no economic or other gain was obtained over what was given by an ordinary

Mr. Isherwood.

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Mr. Isherwood. steam-boiler and furnace, having the heating surface of the Beaufumé producer and boiler combined, and in which the same weight of similar coal was consumed per hour. It was owing to the failure of the principle, and not to the fact that the heating-surface in use was inconveniently divided between a producer and a boiler, which caused the definitive abandonment of the Beaufumé system; and the same result would follow with all other gas-producers used for the manufacture of gaseous-fuel. In fact, all the various kinds of gas-producers for the manufacture of gaseous-fuel were essentially the same, differing only in unimportant practical details, and one kind was about as good as another; they would all give the same economic result when tried under proper comparable conditions. The facts seemed to have been entirely overlooked that the air was in the gaseous state, that it was in complete contact with the molecules of the solid coal with which its oxygen was to combine, and that the two could not possibly be brought into closer contact or more intimate connection by previously gasifying the coal. Also, that as in the ordinary furnace of a steam-boiler the volatilizable hydrocarbon portion of the coal was always burned in the gaseous state after mixture with air, no producer was needed to bring about this effect, because the temperature of volatilization of these hydrocarbons was less than their temperature of ignition. Gas-producers had their true use in the manufacture of water-gas for illuminating purposes, to which alone their future would be limited. The present state of the subject strikingly manifested the danger and folly of abstract investigations of physical phenomena, of determining, *à priori*, how Nature might, could, would, or should act, instead of putting the direct question of how did she act, by means of sagaciously devised experiments. The phenomena of combustion could be investigated only by experiments on a large scale; and not by imagining conditions and results, the former probably having no existence, and the latter not following even if the conditions did exist.

Professor Dr. Lunge. Professor Dr. G. Lunge did not suppose that the Author intended his Paper to be an exhaustive treatise on gas-producers, and it would not be right to blame him for having left out a considerable number of the different descriptions of gas-producers constructed in all industrial countries. Still it might have been desirable not to omit some of the most important and successful of the new gas-producers, as Haupt's, and especially Liegel's (the former was not mentioned at all, the latter merely by name). Although a reference to regenerators or recuperators did not seem to have

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entered into the Author's plan, yet he could not understand how he could have omitted mentioning that large and important class of producers, which embodied some of the principles of recuperators, especially the heating of the air without any special apparatus, such, for instance, as those of Bicheroux and of Boëtius. Without that the history of gas-producers was hardly intelligible. The Author also had not mentioned the fact that, at least on the Continent, there now seemed to be a general tendency towards attaching the producer to the furnace itself; that was converting by suitable modifications the ordinary fireplace into a gas-producer. This effected a great saving in the cost of plant and in space, and it especially prevented the loss of heat by radiation on the passage of the gas from the generator to the furnace. Lastly, the Author seemed not to have appreciated the great importance of continuously removing the incombustible residue in the form of a liquid slag, for which Liegel's producer would have afforded one of the best examples. In short, Dr. Lunge thought that the Author did not bring the subject of gas-producers forward in all its more important bearings. Professor Dr. Lunge.

Dr. Lunge refrained from criticising details, except two points, both occurring on p. 7, where the Author repeated fallacies which he was sorry to meet with constantly, although there could not be any doubt upon the point, and although they were of a very elementary nature. The Author, by a method of calculation of which he could not follow the details, arrived at the conclusion that the use of steam in the Wilson producer showed "an apparent saving" of 6·9 per cent. in theoretical heating-power. Now it was one of the fundamental laws of nature that it was impossible to produce energy from nothing, and hence the hydrogen formed by the decomposition of steam must have been formed at the cost of the calorific power of some of the carbon. In whatever roundabout way the calculation might be made, it would be impossible to show an increase of heating-power, any more than to gain motive force by any arrangement of wheels and levers interposed between the primary motive power and the place where it had to perform work. Steam, therefore, could not produce any saving of heat, apparent or otherwise, so far as theoretical calculations were concerned, and the Author dealt only with such in the place mentioned. Whether the introduction of steam in given cases was a practical benefit or not was quite another question.

The other fallacy was the assertion that a higher temperature of combustion could be obtained by producing gas free from nitrogen. From the context it was evident the Author here

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Professor Dr. Lunge.

meant "water-gas." Now he (like many others) did not notice that the temperature of the flame was always a function of the volume of the gaseous products formed during combustion, and that this volume in the case of water-gas was not smaller, but larger than in the case of ordinary producer-gas; the quantity of nitrogen, moreover, was exactly the same in either case. For whether 12 parts (by weight) of carbon were burned directly with 32 of oxygen into 44 of carbon dioxide, or first with 16 of oxygen into 28 of carbon monoxide, and this afterwards with other 16 of oxygen to 44 CO_2, the 44 CO_2 would be accompanied by the quantity of nitrogen corresponding to the 32 parts of oxygen used, namely, 107 N. Now in order to burn theoretically perfect water-gas, free from nitrogen, according to the formula $C + H_2O = CO + H_2$, the identical 32 parts of oxygen were required, accompanied by the identical 107 of nitrogen, and to this must be added the aqueous vapour formed during combustion, whilst the heat theoretically produced in the case of water-gas was less than in the direct combustion of coal, because the liquid water with which the process commenced must be reproduced in the shape of steam. The combustion temperature of ordinary generator-gas, if it was not allowed to cool down during its passage from the producer, was theoretically equal to that produced by the direct combustion of carbon, for which Dr. Lunge would accept the value given by the Author, namely, 2,716° Centigrade. That of theoretically perfect water-gas, according to the formula, $CO + H_2 + O_2 = CO_2 + H_2O$, would be—

$$\frac{68{,}370 + 57{,}560}{44 \times 0{\cdot}217 + 18 \times 0{\cdot}481 + 2 \times 53{\cdot}6 \times 0{\cdot}244} = 2{,}839°.$$

Such a difference was of no moment, all the more as even 2,716° was above the temperature actually obtainable, owing to the partial dissociation of CO_2 and H_2O. Moreover that difference more than vanished when water-gas, as it was practically made, was compared with ordinary good producer-gas.

He was convinced that the temperature of a flame of good water-gas was much higher than that of coal-gas burned in a Bunsen burner, inasmuch as the water-gas flame for the same amount of gas burned was very much smaller, and therefore the loss of heat by radiation must be correspondingly less.

In one respect he entirely agreed with the Author, namely, that the well-known old type of Siemens producer was very much behind other apparatus of that kind, and that it owed the widely-spread application it enjoyed at one time only to the celebrity

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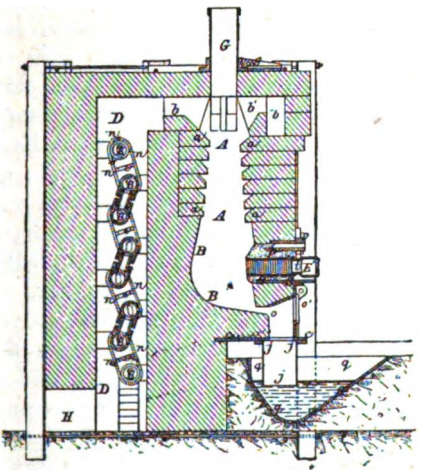
of its inventor, and to his great achievements in other applications of gaseous-fuel. Professor Dr. Lunge.

Mr. W. L. E. McLEAN stated that apart from the drawback of having to feed the "Lancefield furnace" slowly by hand, Scotch coal had been found very unsuitable for it. Mr. McLean.

Mr. E. MINARY submitted a description of his producer with forced blast and fluid cinder, which formed a modification of his double-combustion hearth mentioned at p. 18 of the Paper. The apparatus (Fig. 75) was based on the principle first of converting the fuel into gas, without losing the heat disengaged at the time of the production of the carbonic oxide on the first hearth; secondly, of Mr. Minary.

FIG. 75.



MINARY PRODUCER.

volatilizing the hydrocarbons by the heat of the gaseous current from the first hearth; and thirdly, burning the gases in a combustion-chamber of firebrick exempt from all contact with solid fuel, the walls of the chamber attaining a very high temperature. This chamber had to be sufficiently roomy to allow of the gas occupying at least two seconds of time in passing through it, so that the reduction of its speed of transit caused the formation of eddies, which induced admixture with the air let in from the top of the chamber. In this way complete combustion was effected with only the volume of air strictly necessary, at the same time that the maximum temperature was obtained, and that all the heat due to the chemical composition of the fuel was set free. The apparatus was so designed that these operations were conducted

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Mr. Minary. automatically, the fuel being fed to the generator by gravity, as required, to fill up the gaps produced by combustion, and by the separation of the mineral and other refractory constituents of the fuel which passed off after liquefaction by a small circular orifice *o*, at the bottom. This liquefaction was obtained by the addition of limestone to the fuel, converting the refractory constituents of the latter into fuzible silicates under the influence of the high temperature of the furnace. No attention was necessary beyond that of a man to charge the fuel to the hoppers at fixed intervals, and to remove the slag, already broken up by falling into a water-space below the hearth. The generator was of firebrick, having an interior compartment, A A, of rectangular section, larger at the bottom than at the top, in order to facilitate, by spreading, the descent of the fuel. The walls of this chamber, instead of being flat, were formed of chamfered sections, *a a*, in order to induce during descent the movement of the fuel from side to side alternately. The part immediately below this compartment, B B, formed the hearth of the producer. Its peculiar shape was designed to lessen the effect on its walls of the intense heat; at the bottom it was formed like a crucible, and it was furnished at one side with a small circular opening, *o*, through which the liquid slag passed on its way to the water-space, *q q*. The hearth, B B, was furnished with one or more tuyeres formed of tubing, through which flowed a current of cold water. The pipe, E′, conveyed the blast, previously heated by passing through the pipes E E. The blast was produced by any suitable apparatus capable of delivering the air at a proper pressure. The chamber, D D, was rectangular throughout its entire height. It was traversed in the direction of its larger diameter by eight cast-iron pipes, E E, arranged in a zigzag direction, so that the descending current of gas from the producer was obstructed by each pipe, and so obliged to form eddies, resulting in the more efficient heating of the air-blast carried by these pipes. At the bottom of the chamber, D, was an opening, H, through which the gas passed away to the furnaces in which it was to be burnt. The producer was fed by the hopper, G, made of sheet-iron, of which the cross-section formed a flattened ellipse.

The foregoing remarks and illustration related to the producers actually constructed by Mr. Minary, and which had been satisfactorily tested, although the severe depression existing in the French metallurgical industry had temporarily stopped the working of the factories where they were employed. Experience had suggested the following important modifications of the original design, which would be carried out in future apparatus. In

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the system of tubes in the heating-chamber, D D, would be substituted a single pipe placed in the chimney for the escaping gases in the re-heating furnace. The chamber thus disencumbered of the pipes *n n* would be used as a combustion-chamber. In the roof would be formed an air-inlet, 1½ inch wide, covered by a small sliding ⌐¬-shaped casting, serving to regulate the supply, which would ordinarily be that obtained from the inlet opened to the extent of ⅜ of an inch. The reason for this alteration in the design was very curious and interesting. It had been found that air heated to a very high degree did not suit the producer. The original disposition of the tubes heated the air to above 400° Centigrade (752° Fahrenheit)—a temperature so high that the slag partially volatilized, and the result was volatilized matters opposed the combustion of the gases in the combustion-chamber. It was noticed that when the gazogene was worked with an air-blast exceeding 150° Centigrade (302° Fahrenheit) the pipes became white hot, and the gases arriving from the combustion-chamber, instead of being clear and perfectly transparent, were dull and opaque. Combustion no longer operated upon the whole of the gas, but only on the upper strata, and the temperature of the remainder became stationary at a point below the welding-heat of iron. The resemblance remarked between these conditions and those of the cases of blast furnaces working gray highly carburetted iron led Mr. Minary to analyse the dust deposited under such circumstances from the producer-gas; it was extremely fine and white. It contained silica, alumina, lime, magnesia, and traces of iron, that was to say, all the elements constituting slag. When the heating apparatus was superseded by a single pipe delivering the air at 100° Centigrade (212° Fahrenheit) complete combustion of the whole gases was obtained, and a heat produced in the combustion-chamber sufficient for the welding of iron bars. Minary had not yet published this discovery, which explained how it was that the gas given off by blast furnaces working grey pig were so feebly combustible. Mr. Minary.

Dr. N. H. SCHILLING remarked that the Paper was mainly a summary of gas-generators, without dealing with their working or giving the result of experience acquired from a practical acquaintance with them. His personal experience consisted specially in gas-generators and regenerator works for making gas for lighting purposes, which had barely been mentioned in the Paper. It seemed that the Author had taken no notice of the work done in Germany. The scientific researches of Dr. Bunte opened up new Dr. Schilling.

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Dr. Schilling. ground.[1] Dr. Schilling's communications on the construction and working of furnaces in Germany, and their views on heating by gas, would be found in detail in the "Journal für Gasbeleuchtung." He had an impression that the heating of retorts in gas-works was regarded in England from a slightly different point of view to that in Germany.

Mr. Stevenson. Mr. G. E. STEVENSON remarked that the Paper was a good description of the various producers or generators which had been designed for the purpose of gasifying coal or coke, but it was not of great interest to the gasworks manager, as it dealt principally with apparatus designed for other purposes than heating gas-retorts. The reference at the close of the Paper to the producers introduced into gas-works did not give an adequate idea of what had been done in this direction. It was not correct to say that "the principal (producers) are those of Klönne, of Dortmund, of Valon, of Baecker, the Dessau or Didier producer used in Dresden, and the modification, or so-called modification, of the Siemens circular producer used in the Corporation Gasworks at Glasgow." It could not be claimed for the Valon furnace that it was one of the "principal," for this furnace contained nothing of individual interest, but was a copy of that introduced by Mr. George Livesey at the South Metropolitan Gasworks (which itself was, in principle, taken from that of Liegel), but with an extended development of regenerating flues. As a matter of fact these furnaces, as well as those of Liegel and Klönne, could not be called "producers," as they were nothing more than deep furnaces, in which the combustion was completed at the top of the furnace by a fresh supply of air being introduced at that point, the primary supply at the bottom of the furnace being insufficient for entire combustion of the fuel. As regarded the application of gas-generators to the retort-settings in a gasworks, the principal difficulty was the disposal of the residue. For this reason, two main differences arose in the method of gasifying, the one in which it was attempted to remove the residue, by melting it and running it out at the bottom of the furnace, the other in which the constructors sought to prevent the formation of clinker, and to reduce the residue to soft ash, which could be removed without difficulty. The first generators applied to gas-retorts in Germany were of the former kind, namely, those of Didier, Hasse, and Liegel. In the two first success was not obtained, because the destruction of

[1] Some account of these researches will be found in the Min. Proc. Inst. C.E. vol. lxv. p. 393, and vol. lxxiii. p. 430.—SEC. INST. C.E.

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the lining of the generators was too great. The furnaces had large horizontal openings at the bottom on the level of the hearth, and the fireclay lumps surrounding these openings were quickly destroyed. Liegel claimed to treat successfully the clinker on this principle, by so constructing his furnace that the partially melted clinker remained as a thin stratum, covering and protecting the brickwork, and the superfluous clinker escaped from a narrow slot at the bottom of the furnace, falling directly down into an ash-pit, and not, like the Didier, remaining on a hearth in proximity to the furnace sides until drawn out by rakes. It must be stated that the Liegel furnace, though successful with the coke from certain classes of coal, was not adapted to those, the coke from which produced a clinker difficult to fuse. The other system of dealing with the residue was originated by Dr. Schilling assisted by Dr. Bunte at the Munich Gasworks. These gentlemen placed a flat pan or tank containing water beneath the grate of the generator, and evaporated the water by conducting the waste gases under the pan, after they had left the regenerator. By this means they succeeded in preventing the formation of clinker, the residue consisting of friable breeze or ash, which could easily be removed. Other constructors, such as Klönne, adopted the use of water and steam for the same purpose. The Munich furnace and the "Didier" furnace were true generators or producers, being separated from the furnace where the object to be heated existed; the others, excepting the Siemens producers, were constructed within the retort-setting itself, and formed part of it as an ordinary furnace. The main object in the application of gaseous-firing to retorts was the adaptation of a suitable system of regeneration, which should combine efficiency with economy in the construction, and not occupy an unnecessary amount of space. For this latter reason the regenerating flues had been, in all the systems, placed below the retort-setting on either side of the furnace. The systems of Schilling and Klönne possessed the most complete regeneration, and gave the most economical results in regard to consumption of fuel. That of Liegel was less expensive, and gave a high heating power, but the proportion of fuel used was somewhat greater.

Mr. Stevenson.

Mr. W. S. Sutherland.

Mr. W. S. Sutherland was constrained to criticise some parts of the Paper. In a communication to the Society of Chemical Industry[1] he endeavoured to classify gas-producers according to their natural development. He feared that water-gas producers, in which the fuel was put into retorts and raised to the required

[1] Vol. ii. 1883, p. 62.

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Mr. W. S. Sutherland. temperature by heat applied externally through the retort-walls, and then immediately to the fuel, would not come into favour. The temperature was too high for the stability of the complex conditions which would have to be present, and the only use of the external fire would be to act as the steam-jacket did to a high-pressure steam-cylinder with cut-off valve. From actual experience on a large scale with water-gas producers Mr. Sutherland still held the opinion expressed in his Paper just referred to, only supplemented by their expansion caused by some improvements in the methods and apparatus to be used. These had been thought out and patented, and would in all probability come into immediate use. From referring to the Author's description and criticism of his producer of 1874, Mr. Sutherland would say that this patent embodied the results of actual work carried out in 1872 and 1873, and that he and his friends had at that time very distinct ideas and views as to the collection of by-products from the gas. Probably if he had stated all the possibilities suggested by those ideas he would have been laughed at, but he had been working at the recovery of these products ever since, and at the elimination of nitrogen from the gas, and if it was complained that he had not given more detailed descriptions and drawings in the patent of 1874, he would submit that they were sufficient for their purpose.

Mr. Tervet. Mr. R. Tervet observed that the chronological synopsis of the various types of gas-producer, which had from time to time been introduced, proved that the only one capable of general application, and useful for dealing with fuel at present valueless for ordinary purposes of firing, was the Wilson gas-producer. Although it fell far short of the ideal, yet it realized all that was claimed for it, and must, until supplanted by a better, gradually make its way wherever gaseous-fuel was applied. Indeed, from the principle of working, and the method by which the results were sought to be obtained, it might be said to be practically perfect, so far as a knowledge of the reactions within the producer permitted a determination; but so long as it was necessary to introduce such a large volume of atmospheric air as a reagent for the production of carbonic oxide, it was useless to expect any great improvement from a mere modification of the structural features of the producer itself, while the principle of working remained unchanged. It was to be regretted that the Author had not investigated the chemistry of the subject, and dealt with those reactions which regulated the production of gas from coal-slack; as also the formation of ammonia from the potential nitrogen present in the

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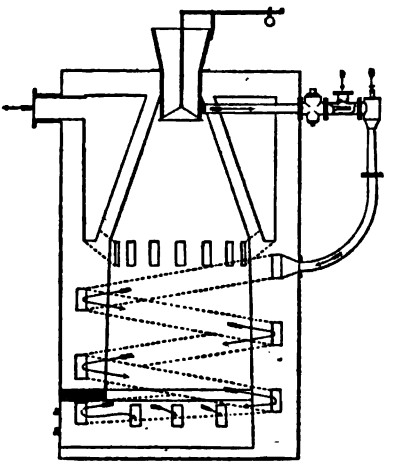
coal. All writers on this subject, with the exception of Young and Beilby, had carefully avoided treating this matter as a practical and scientific problem. As pointed out by the Author, Young and Beilby, by a mere modification in the composition of the reacting atmosphere, had been able to obtain practially all the nitrogen in the coal in the form of ammonia; but this modification of the gas rendered it unsuitable for nearly all purposes to which gaseous-fuel was applied. As an attempt to realize the advantages of the Wilson gas-producer, and also that of Young and Beilby, without any change in the composition of the resulting gas, he had constructed an experimental gas-producer. It would be observed

Mr. Tervet.

FIG. 76.



TERVET EXPERIMENTAL PRODUCER.

from the arrangement of the apparatus (Fig. 76) that the destruction of the coal was divided into two stages. Suppose the producer to be at work, and that the upper cone was full. Now, as the total contents sank within the body of the producer, there must remain a space between the mass of caked coal and the wall of the retort. The new charge passed into this space in a comparatively thin layer all round, where it underwent incipient distillation, yielding gas and tar. These, in the form of vapour, were drawn out from the retort by the steam-jet, and together with a regulated supply of air were passed several times round the body of the producer, and finally into the culvert underneath, and thence through the incandescent coal, where the steam and tar were

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Mr. Tervet. decomposed; and the gases were conducted away in the ordinary manner. It would further be seen that an equilibrium of exhaust and pressure must be set up by the action of the steam-jet within the producer. Thus the upper cone or retort was always under exhaust, while the lower portion or body of the producer was under pressure. The apparatus worked admirably, destroying all the tars; it yielded a very pure gas, and he was satisfied that the yield of ammonia was far above that obtained by the Wilson gas-producer.

Mr. Thwaite. Mr. B. H. THWAITE said that if solid carbonaceous fuel could be perfectly oxidized with exactly the requisite quantity of air, there would be no reason for its partial oxidation in a gas-producer. Unfortunately, in ordinary practice two or three times the quantity of air theoretically necessary was required to completely oxidize solid fuel in ordinary furnaces, and even with this excessive quantity of air it was almost impossible, especially when the fuel was of a richly bituminous description, to prevent the escape of unoxidized fuel. With this threefold quantity of air there was an enormous preponderance of the diluent gas nitrogen, which not only absorbed a great quantity of heat, both in the partial oxidation by air traversing the fuel, and in its complete oxidation by the air passing over the fuel, but the envelope of inert nitrogen very seriously hindered the action of oxidation. The moisture in the air was also an element which absorbed part of the heat generated. The vapour in passing over a solid fire was not sufficiently heated to produce dissociation; but even if it were dissociated the heat absorbed in this action would not be regained, as the gases passed away in a separate condition as H and O. Thus carbonaceous-fuel in its solid form was not prepared for complete and economical oxidation. The question was therefore of its partial oxidation with a minimum quantity of atmospheric air. If steam was allowed to pass into and through incandescent carbonaceous-fuel, it was dissociated. By this there existed not only the element of oxidation at the expense of a certain number of calories, which in the complete oxidation of the carbon and the reoxidation of the hydrogen was returned, but there was in addition the saving of heat due to the absence of nitrogen. With the partial oxidation of the solid fuel in the gas-producer, the quantity of air for the production of the carbon monoxide (CO) and the balance of oxygen could be almost exactly supplied to the now gaseous fuel, for its complete oxidation to carbon-dioxide (CO_2).

In order to use steam economically, a certain degree of oxidation

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was necessary to replace the heat absorbed in its dissociation. In one type of gas-producers this was effected by supplying air alone to the producers until there was sufficient heat to allow the supply of steam to be introduced; then both steam and air were allowed to enter together. In another type of gas-producer there were alternate periods of air alone followed by steam by itself. Half the resultant heat generated in the first period was stored in chambers filled with refractory material, through which in the second period the steam passed, and became heated to the degree necessary for its proper dissociation in passing through the incandescent carbon in the producers. This latter type was the most theoretically correct, and yielded the purest and richest gas, containing by volume— Mr. Thwaite.

	Per cent.
H	50
CO	45
CO_2	2·5
N	2·5
	100

In the preliminary blowing-up of the carbon with air, nearly all the sulphur in the fuel was volatilized and removed. This was the type of producer which Mr. Healey and he were applying to the reduction of iron ore in the blast-furnace. The valvular arrangements of these producers were the result of considerable experiment and much thought. In the production of heating-gas, he had found that each of the distinct varieties of coal required a distinct process and apparatus to effect properly its partial oxidation. With some of the anthracite coals the air had to be introduced at different levels in the depth of the fuel, otherwise only a small layer, in proximity to the point of influx of the air-supply, would remain incandescent. And if the air-blast was too powerful, and the heat of the fuel too low, part of the anthracite was removed in a finely divided condition and deposited in the flues. In using bituminous or semi-bituminous coal it was found that unless the volatile hydrocarbons were made to traverse the incandescent portion, and thus become partially oxidized, rearranged, and fixed, they were deposited in the flues in the form of tar; consequently, in the best arranged producers the outlet flue for gas from the producer was at the level of the incandescent portion of the fuel. Producers should be so constructed that it would be impossible for the air-blast to pass up the inside walls and mix with the combustible gases. This could best be pre-

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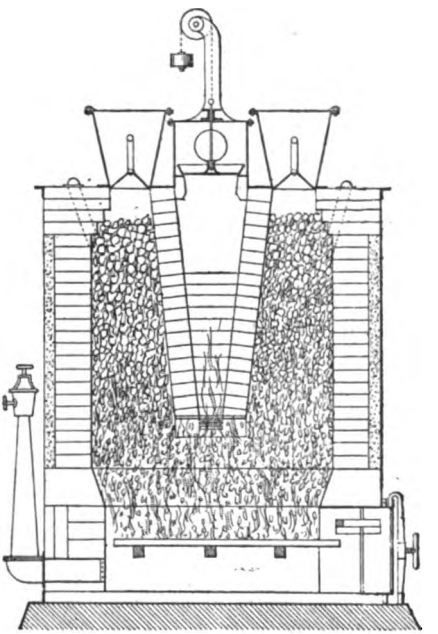
Mr. Thwaite. vented by arranging the gas outlet in the centre of the producer, as in the Thwaite twin gas-producer (Fig. 77). There should be no serious internal projections on the inside walls on which the clinker could hang, otherwise there would be the inevitable scaffolding of the clinker. The conduits for leading the gas from the producer should be formed of refractory and anti-diathermic material—fireclay bricks were the best. The method of using iron tubes not lined with bricks cooled and condensed

FIG. 77.



THWAITE TWIN GAS-PRODUCER.

the gases, not only absorbing their high sensible heat, but producing the tarry deposition of the rich hydro-carbons, the smallest trace of which once formed became a nucleus for a larger accumulation of tarry matter. If the conduits were constructed of bricks or tubes lined with refractory materials, there was rarely any serious deposition of tar; and if it was formed it could easily be burned out by allowing air to enter through a hand-hole in the flue. For gas-valves, Mr. Thwaite had found those of the mushroom-type the most serviceable. For charging hoppers

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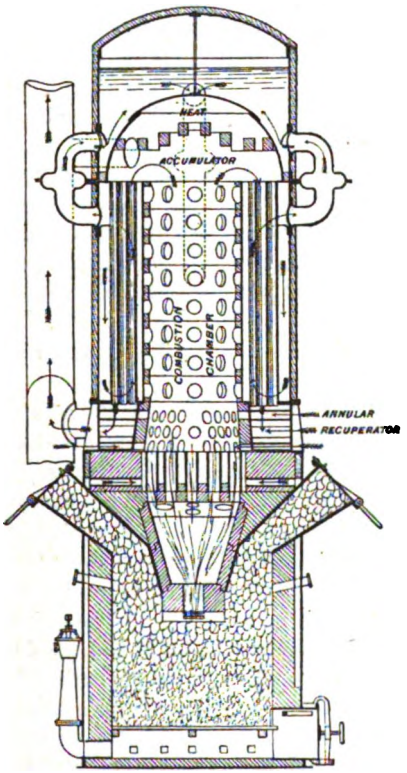
if the producers were of the Siemens type, the simple plug-hopper, as in the producers of the Bower-Barff furnace, were serviceable; but there was less escape of gas with the ordinary Siemens balanced hopper. The bell-hopper arrangement, with sliding lid, was a good type for forced-blast producers. For pure water-gas producers, at Trimsaran, an arrangement similar to the cross- Mr. Thwaite.

FIG. 78.



THWAITE GAS-PRODUCER AND STEAM GENERATOR.

bar retort doors had been adopted, so that when the pressure of steam, which was 5 lbs. to the square inch, was turned on, there would be no escape of gas. Single analyses of producer-gases were practically useless, as even in the best arranged producers the character of the gas varied with the depth of the fuel, its physical arrangement, the proportion of the fuel to the clinker,

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Mr. Thwaite. and the pressure of air and steam-blast; consequently, at least ten different analyses should be taken, to obtain a proper representative value of the gas produced. He had found the following clinkering arrangement to be the best, namely, simply driving false or clinkering bars through the fuel just over the ordinary bars, and then withdrawing the latter, allowing the clinkers to fall into the ashpit. The fire-bar arrangement admitted of the air and steam-pressure being more equably applied to the fuel than any of the other arrangements, and consequently promoted the generation of gas of a more uniform quality.

In the Thwaite Twin gas-producer, Fig. 77, he had arrived at the following results:—

1st. The production of a more uniform and regular supply of gas from one producer.

2nd. The prevention of the possibility of the air-blast from passing unaffected through the fuel and mixing with the gases, and the reduction of the probability of explosions.

3rd. Compelling all volatile hydrocarbons to traverse the incandescent portion of the fuel before they could mix with the carbon-monoxide gases.

4th. The feeding of the fuel into the producer without seriously affecting the supply of gas.

It would be seen that this gas was withdrawn very much from the centre of the producer by a hollow dividing-bridge, which dipped below the upper surface of the incandescent portion of the fuel. The hollow bridge practically divided the producer into two divisions, each having its separate charging-hopper. The producer had been adapted for firing the Thwaite Steam-Generator (Fig. 78). Part of the air-blast supply passed from under the producer fire-grate through cavities in the walls, and surrounded a group of tubular gas-burners immediately over the producer. The heat of the products of combustion, after traversing the combustion-flue, heat-accumulator chamber and tubes of the steam-generator, passed through an annular air-recuperator to the chimney. The secondary air-supply for the combustion of the gas was conducted through this recuperator. Healey's gas-producer was the development of the form used by him in the construction of the Elba Steelworks, Gower Road, in 1872, for firing open-hearth steel-melting furnaces. The firebars were in two lengths, resting on a hollow cast-iron bearer placed centrally across the ashpit of the producer, and through which the steam and air-blast pressure entered. By means of a screw-arrangement, workable from the outside, the front bearer-bars carrying the

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fire-bars could be lowered during clinkering to facilitate this process. Mr. Thwaite.

Mr. T. URQUHART directed attention to the prevalent use of wood for gas-making for metallurgical purposes in the interior of Russia. He had seen, at the Bryansk Steelworks in the Government of Orel, six regenerative gas-furnaces for making Siemens-Martin steel, besides many other gas-furnaces for heating ingots and welding iron, all worked by wood-gas alone. He had seen the same on a lesser scale at the Sormova Ironworks in the Government of Nishninovgorod. So far as he could notice, the wood-gas producers were of the ordinary Siemens construction. The Grazi-Tsaritsin Railway, which was more than 460 miles long, and equipped with one hundred and forty-three locomotives, was the only railway in existence on which petroleum-refuse was used, to the complete exclusion of all other kinds of solid fuel, for locomotive purposes.[1] Plans had been prepared for the introduction of gas in the various furnaces in the central workshops of this line. Meanwhile trials were in progress for working the ordinary reverberatory welding-scrap furnaces with liquid fuel, without gasifying the fuel in a separate producer; the results so far, even with very simple appliances, had been much superior to these attained with coal as fuel. No doubt liquid fuel could be easily gasified in a simple special producer. Mr. Urquhart.

Mr. C. WITTENSTRÖM remarked that the Wilson generators, with their latest improvements, were probably the best for producing the so-called Siemens gas. Since 1881, Mr. Wittenström had been using oil as the fuel, in heating furnaces for his new process of manufacturing wrought-iron castings (Mitis). Mr. Wittenström.

Mr. LEWIS T. WRIGHT regretted that the Author had not investigated the thermo-chemical aspect of the gaseous-fuel question, and thus had avoided arriving at certain erroneous conclusions. In stating that a higher calorific intensity might be obtained with the gaseous-fuel, the Author instanced the fact that when carbon was burned in air a temperature of 2,717·6° Centigrade was arrived at, whereas when carbonic oxide was burned in air a temperature of 2,982° Centigrade was reached. This had nothing to do with the present nor with the immediate future of gaseous-fuel, as no means were known whereby in practice undiluted carbonic oxide could be produced. Generator-gas, having a theoretical composition of 34·3 per cent. of carbonic oxide and 65·7 per cent. of nitrogen, would give a flame temperature of Mr. Wright.

[1] Institution of Mechanical Engineers. Proceedings, 1884, p. 272.

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Mr. Wright. 1,915° Centigrade only. The advantage to the extent of 6·9 per cent. in cases where steam was used along with the primary air-supply to internal-combustion producers was purely illusory, as the heat required to vaporize the water into steam had not been taken into the calculation, which heat was not afterwards recovered.

	Calories.
For instance, the 97 kilograms of carbon in conversion to carbonic oxide would afford	231,103
The carbonic oxide thus produced by burning to carbonic acid a further	552,657
	783,760

In the other case—

85 kilograms of carbon into carbonic oxide by air . .	202,513
12 „ „ by reaction $C + H_2O$ (fluid) = $CO + H_2 = 28,590 - 68,360 =$	39,770
	162,743
97 kilograms of carbon in the form of carbonic oxide burned into carbonic acid	552,657
2 kilograms of hydrogen burned to water vapour	57,560
	610,217
	772,960

—or a loss, as compared with the first method, of 10,800 calories, or 1·38 per cent. These 10,800 calories were required to convert the 18 kilograms of water into steam to feed the generator. It must always be remembered that in these steam-fed generators the steam required fuel for its production, and that the heat expended in evaporation was not afterwards recovered. The reaction in question was

$$C + O = CO \ldots\ldots + 28,590 \text{ calories},$$

or nearly 30 per cent. of the total thermal efficiency of carbon oxidized to carbonic acid. The Author, in mentioning that illuminating-gas had a higher calorific value than ordinary water-gas, but not higher than the gas made in the apparatus of Lowe and Strong, should have stated that the Lowe gas was illuminating-gas (it was so mentioned in the Appendix in the notes on the analyses Nos. 50 and 51). Mr. Wright had on various occasions endeavoured to gasify coal by the method 5, but always with unfortunate results, amongst them the trouble of obstructed pipes. The Allen-Harris process was founded on the principle of gasifying anthracite in retorts, similar to those used in ordinary gasworks, by means of

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steam; but it was probably inferior to those processes where water-gas was manufactured in the so-called "gazogenes," by "blowing" the fuel alternately with air and steam. The fractional method of burning carbonaceous-fuel, based on the property carbon possesses of forming two oxides, by passing one fraction (one-half in the case of a partly carbonaceous-fuel) of the supply of air requisite for the combustion of a given quantity of the fuel in a given time, through an excess of the fuel placed in a deep firebrick chamber called a generator, and reserving the remainder of the requisite air-supply for the final oxidation of the carbonic oxide produced by the first stage, had of course other advantages than the possibility of thereby effecting the combustion of the fuel with (closely) the theoretical air-supply. Heat from waste products of combustion could be transferred to the secondary air-supply, thus increasing the temperature of the final stage of combustion. There were doubtless considerable advantages attending the introduction of a limited quantity of steam to generators for the absorption of heat. The reaction between the incandescent coke and the steam kept the generator cooler, and prevented the fusion of the ashes into more troublesome slag or clinkers, which forming at the base of the generator choked up the air-passages. This absorbed heat was afterwards recovered in the combustion of the resulting carbonic oxide and hydrogen. The fractional method of fuel-combustion also admitted of the recovery of tar and ammonia, by the cooling and treatment of the gases formed in the first stage; but it must always be a grave question whether the inevitable losses of heat attending this process would not largely neutralize the value of the recovered products. The recovery of these from blast-furnaces was a rather different matter; but, in any case just now, their utmost value could hardly be more than 1*s.* 6*d.* per ton of coals, without deducting anything for expenses of their collection, wear and tear of plant, and interest on the capital outlay. Mr. Wright was of opinion that, at the present values of tar and ammonia-products, their recovery from blast-furnaces must be a certain commercial loss. It would be interesting to know what financial profit had attended the experiences at Gartsherrie and Langloan. A little while ago gas-manufacturers were receiving high prices for tar and ammonia, and inflated by a success not due to their exertions, but to outside influences in many quarters, they indulged in vain boasts which produced much excitement in the public mind. Exaggerated statements were rapidly promulgated, even in technical periodicals, respecting enormous profits

Mr. Wright.

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Mr. Wright. made by coal-gas concerns out of residuals, and how the gas could be given away. Of course, these inflated ideas caused the influx of capital into various schemes for the production of "residuals" from coal, and even coal-gas itself. The output of these schemes coming just on top of a bad trade, when all values were diminishing, and demand was decreasing, and whilst owing to other causes the consumption of gas was yet extending, and consequently the tar and ammonia from gasworks were increasing in amount, caused a drop in the values of gas-products, from which the market would be long in recovering.

Mr. Rowan. Mr. F. J. ROWAN observed that, while admitting that the addition of the numbers representing the calorific value per 100 litres to all the analyses would increase the value of the Table in the Appendix for the purpose mentioned by Mr. Beilby, he felt that, as the analysts had not in each case furnished these figures, it would not be proper for him to add them, as he published with the analyses the names of their authors. They could, however, easily be found from the following data :—

1 litre H	= 3,106	gramme	Centigrade	units.
1 „ marsh gas	= 9,587	„	„	„
1 „ CO	= 3,130	„	„	„

He did not, however, admit the fairness of Mr. Beilby's comparison of Analysis No. 47 with Nos. 15 and 26, for the reason that, as shown by Mr. Beilby's Paper in the "Journal of the Society of Chemical Industry," vol. iii., p. 216 *et seq.*, and as stated in the notes appended to the analyses, *ante* p. 55, gas of the composition of Analysis No. 47 had not been produced. That analysis was merely an estimate of the extent to which No. 46, which was the gas produced at Oakbank, might be expected to be improved, provided the air and steam-supply were accurately proportioned, which was a matter not very easy to realize in practice. Mr. Isherwood's estimate of the advantages to be derived from the combustion of gaseous as against solid fuel, restricted "the possible improvement in the economic combustion of coal" to the narrow limits represented by a saving of from 2·5 to 8·3 per cent.; and as regarded the loss occasioned by reduction of temperature due to the presence of an excess of air in furnaces fed with solid fuel, he remarked that it could "not be lessened by any previous conversion of the coal into combustible gases and then burning them" with air. He was, however, proved to be wrong in these ideas by the many recorded results of economy of fuel obtained in many heating operations by the use of gas. The reasoning founded upon

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Bunsen's experiments was not sound, because these experiments did not strictly apply to totally different conditions as regarded the abstraction of the heat produced by combustion, as compared with those which were present in Bunsen's apparatus. Regarding the trials of the Beaufumé apparatus at Cherbourg, Mr. Isherwood had evidently not observed the results communicated by the late Professor Rankine to the Institution of Engineers in Scotland, which showed an economic gain of above 30 per cent. over what was given by an ordinary steam-boiler and furnace, using the same quality of coal as the gasifier. The Paper could not, as Dr. Lunge observed, be supposed to be an exhaustive treatise on gas-producers, if those fireplaces which were attached to individual furnaces, and which in their action resembled separate gas-generators, were included under the general title, these, with the exception of Ekman's, having been excluded from the Paper. This was a large class in itself, including, besides Haupt's and those of Bicheroux and Boëtius, those of Ponsard, Gorman, Smith-Casson, Price, and many others; and the consideration of such appliances could not be treated properly apart from the subject of the furnaces of which they formed an integral portion. It was questionable if the heating of the air used for combustion in the producer could be of any benefit. The use of recuperators for heating the air used in the combustion of the gas in furnaces was another matter, and belonged to the subject of the use of furnaces, so that it was quite possible to consider apparatus for producing gas apart from the subject of recuperators. It might be convenient under some circumstances to have a gas-producing fireplace for each furnace, but it was certainly impossible that this plan could compare favourably with that of the use of separate producers for general application, in respect of cost or of space. The cost and size of furnaces must be increased by the addition of such fireplaces, and the gas-producing plant was not available for working any other furnace than the single one to which it was attached, so that there could be no distribution of gas over works, and in the event of some furnaces not being used, the gas-producing plant and the outlay represented by it were unproductive. On the other hand, the fuel had to be distributed to many points, at each of which stokers must be provided, instead of both being concentrated at one place, and thus labour charges were also increased. It had not in the past been found advantageous to the working or life of gas-producers to raise their temperatures of combustion sufficiently high to melt the ash, apart from the inconvenience of mixing fluxes with the fuel, which that plan entailed.

Mr. Rowan.

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Mr. Rowan. It was possible that Mr. Liegel or Mr. Minary might have made some improvements in this direction, but that remained to be proved by practical work. Both Dr. Lunge and Mr. Lewis T. Wright had criticised adversely his employment of Schöffel's figures, showing the effect, as regarded the working of the producer itself, of the use of steam and air instead of an air-supply exclusively; but the criticism was unnecessary, because he did not enter fully into the thermo-chemical data connected with the question of gas-production and use. All that the figures were used for was to show that, considering merely the question of the two methods of working producers, and ignoring for the moment the question of the supply of steam, there was no disadvantage in employing steam. In fact, as producer-gas had almost always to be conducted some distance, and lost heat in this operation, there was an advantage in the use of steam arising from the lower temperature of the gas produced when it was used. It might have been added that, in practical working, the use of steam was advantageous on account of its reducing the temperature at certain parts of the producer, and facilitating the handling of ash and clinker. Dr. Lunge, Mr. Wright, and Mr. Coleman also referred to his employment of Mr. Galloway's equations, in terms similar to those used by Mr. Head on that subject. He could not, however, perceive how they considered it fair to insist upon a comparison between pure carbon and producer-gas, while they objected to one —which was certainly more just for the purpose for which it was employed—between pure carbon and pure carbonic oxide. If accuracy were their aim, why did they not compare the combustion temperature of producer-gas with that of coal? He dissented from Mr. Wright's opinion that these equations had no bearing upon the future of gas-production, because he maintained that it was possible to produce heating-gas which approximated more nearly to pure carbonic oxide than the present quality of producer-gas did; and this opinion was not subject to Dr. Lunge's interpretation, that by carbonic oxide he meant water-gas. In that event these equations showed that a temperature superior to that which could be obtained from solid fuel might be secured by the direct combustion of such gas with air, apart from assistance by recuperation. In fact, Dr. Lunge admitted that the combustion-temperature of even ordinary producer-gas, if it was not allowed to cool down during its passage from the producer, was theoretically equal to that produced by the direct combustion of carbon; and therefore it must be above that produced by the combustion of ordinary coal, so that Dr. Lunge supported the contention. In regard to Mr. Lürmann's Paper, he

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had directed his remarks to the assertions contained in it that the production of carbonic oxide required no heat, and that heat became latent only in the distillation of the hydrocarbons. Mr. Wright, like Mr. Lürmann, had evidently omitted to notice that the formation of carbonic oxide required the transformation of carbon from the solid into the gaseous form, and that, as carbonic oxide was usually obtained from carbonic acid (by its combination with, or dissolution of, carbon), there was also a resulting increase of volume, both of these actions involving the absorption of a considerable quantity of heat. Although it was permissible to speak of the thermal equivalent of the reaction $C + O = CO$, yet that equivalent was merely the difference in amount between the amount due to the complete combustion of carbon to CO_2, and the amount derived from the combustion of CO to CO_2. The direct combustion of solid carbon to CO with disengagement of heat, no CO_2 having been primarily formed, was an unknown chemical process. Mr. Rowan.

With respect to the denomination of the Lowe gas, he had stated in the Appendix, p. 53, that the Lowe gas was illuminating gas, and he presumed that Mr. Wright would admit that even then it remained "water-gas."

He had not seen the producer mentioned by Mr. Coleman, but from inquiries he had learned that its action was not invariably successful. He was in full accord with the remarks of Mr. Clerk, with the one exception that, in the face of the evidence brought forward in the Paper, he could not agree with him in the statement that the late Sir William Siemens originated the gas-producer. No doubt there was a large field for the employment of a good gas-producer in conjunction with the gas-engine. The tar difficulty could be overcome, even if producers of the Wilson type were used, in either of two ways: first, by using coke or anthracite as the fuel in the producer; or, secondly, by washing the gases produced from bituminous coal, neither of these methods offering much difficulty in practice. It might be advisable, in obtaining gas for employment in gas-engines, to use some of the cheaper hydrocarbons as the liquid employed in washing the gas.

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19 January, 1886.

SIR FREDERICK J. BRAMWELL, F.R.S., President, in the Chair.

The Discussion upon the Paper "On Gas-Producers," by Mr. Frederick John Rowan, occupied the whole evening.

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26 January, 1886.

Sir FREDERICK J. BRAMWELL, F.R.S., President, in the Chair.

(*Paper No. 2139.*)

"The Injurious Effect of a Blue Heat on Steel and Iron."

By C. E. STROMEYER, Assoc. M. Inst. C.E.

IN spite of the many excellent qualities possessed by mild steel, and in spite of its extended use for ship-building and for marine boilers, there are still many engineers who consider it a treacherous material. They are able to adduce numerous instances in which steel-plates and bars have failed, in their opinion at least, in a most unaccountable manner. In nearly all such cases a very cursory examination brings out the fact, that the plates in question have been subjected to bending or hammering while hot, and there can be little doubt that, while they were being worked, these plates were at a blue heat or, as smiths and boiler-makers call it, a black heat. It should by this time be well known, that such treatment is the most injurious to which steel can possibly be subjected, and therefore that such failures cannot be properly designated as unaccountable.

Iron, as will be shown, possesses the same peculiarity; but as this material is very much less ductile than steel, similar failures are not so glaring, and when they occur are put down to bad quality.

Plates, both of iron and of steel, have, however, failed without this treatment, although the quality of the material was good, at least according to the usual tests.

The cases which have come under the Author's immediate notice are as follow:—

A steel plate which had been sheared after leaving the rolls, and had not subsequently been reheated, cracked in various places whilst it was being straightened (cold) between the rolls. In another example the backplate of a combustion-chamber of a marine-boiler cracked while cold, after the boiler had been out

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of use for about three months. On testing this steel plate it was found to stand the tensile and temper-bending tests satisfactorily. The iron plate was chipped and proved to be as ductile as could be wished, so that these cases do certainly appear to be mysterious.

It was not with the intention of discovering the reasons of these failures that the present series of experiments were undertaken, but in the hope of gaining additional information on the influence of a blue heat on steel. By an accident, a failure similar to the above occurred to one of the test-pieces. Although, unfortunately, the reasons for this failure are not as evident as could be wished, there are strong indications that one of them has to be sought for in an accidental baking of the steel in a moulder's core-drying oven; but this will be dealt with later on.

The numerous tests in connection with the subject of this Paper consist mainly of bending- and of tensile-tests. The results of the latter are contained in the Appendix, Tables I. and II., and in the diagrams. As they do not throw much light on the influence of blue heat, it does not appear necessary to give a very full explanation; they will therefore be dealt with first.

Table I. contains the results of ordinary tensile-tests on iron and steel. Several test-pieces had, however, been previously subjected to mechanical operations.

It will be noticed that the iron, the hard steel, and the mild steel, all show an increase in their limits of elasticity if previously elongated. In some instances this limit rises above the original breaking-stress, although the ultimate breaking-stress is only slightly affected. The total elongation is reduced while the contraction varies considerably. The rise in the limit of elasticity appears to be influenced both by the original amount of stretching and by the interval of time which elapses between the two tests. If the preliminary stretching is effected with the metal hot, the elastic limit rises higher than if the metal was cold.

A test-piece which had been shortened when cold showed a reduction of the elastic limit, while another piece which had been shortened when hot showed an increase. Two test-pieces which were stretched by flattening underwent a small ultimate elongation. The flattening had very little effect on the elastic limit and the ultimate breaking-stress of the cold-hammered piece, while the hot-hammered piece showed an increase both in the elastic limit and in the ultimate breaking-stress.

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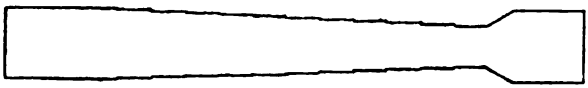
Some peculiar results, noticed during the testing of the above samples, suggested the idea that after the elastic-limit has been exceeded test-pieces do not stretch uniformly throughout their length for every increase of stress. If this were true, then the ordinary stress-diagrams would not be of much value. In order to eliminate all influences which would produce incorrect results

FIG. 1.



Scale ½.

the test-pieces were shaped as in Fig. 1. Each section was very accurately measured before and after testing, and the amount of local elongation was calculated from the amount of local contraction. As the stress per square inch varied for the different sections, the amount of elongation for each stress could be

FIG. 2.

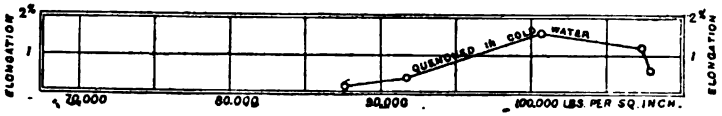
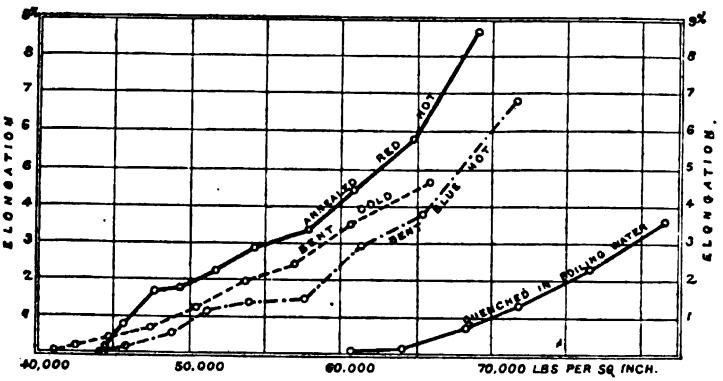


FIG. 3.



SIEMENS-MARTIN STEEL. MEDIUM HARD.

ascertained and plotted. The maximum stress was of course on the smallest section, but as it was difficult to arrest the stretching when the highest load had been reached, the amount of this elongation is incorrect. This value and the maximum stresses

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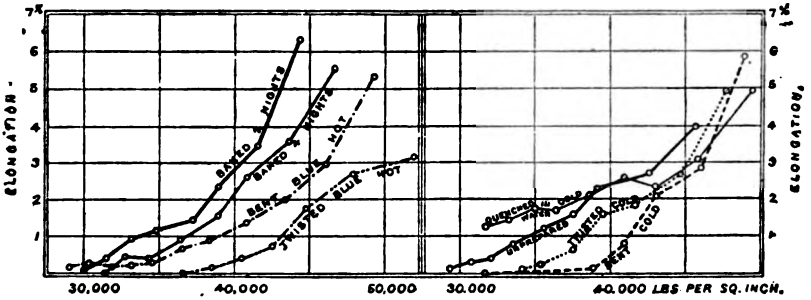
are contained in Table II., while the stress diagrams are included in Figs. 2 to 7.

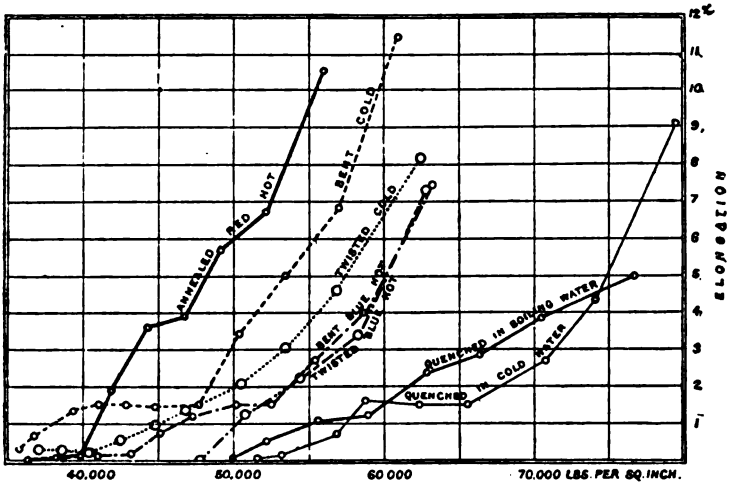
As will be seen, there is a marked difference in the same material under various conditions, but the results are too few for any conclusions to be drawn. Their value is also considerably

FIG. 4.



LOWMOOR IRON.

FIG. 5.



SIEMENS-MARTIN STEEL. MILD.

diminished by want of homogeneity of at least one of the steels. It will be noticed that three lines in Fig. 5 run parallel with the base, which ought to be impossible.

Table III. contains some tensile-tests of steel at various temperatures.

Far more satisfactory results were obtained with the help of

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bending-tests. The earliest, which are contained in Table III., were made at a time when the peculiar influence of a blue heat was less understood than it is now; they are only of value in so far as they embrace various qualities of steel and of iron. The same remarks apply to Table IV.

In order to understand these and the following Tables, it is

FIG. 6.

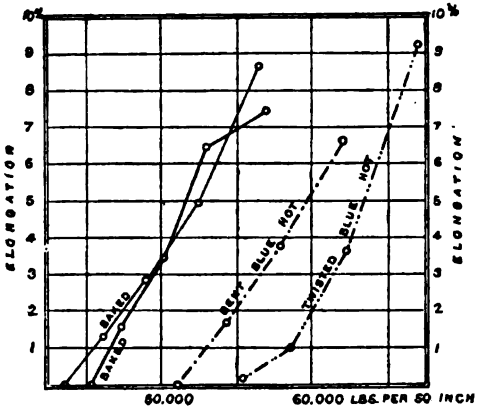
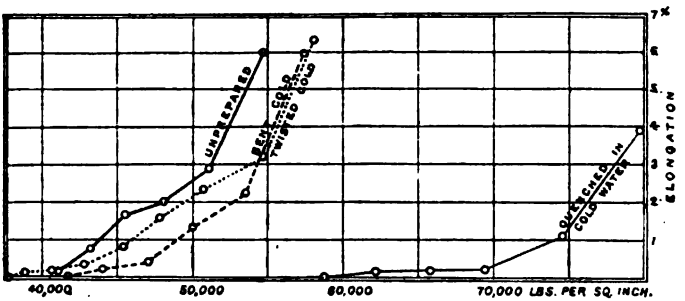


FIG. 7.



SIEMENS-MARTIN STEEL. VERY MILD.

necessary to explain that the test-pieces were never bent double, but first one way and then the other, till they broke.

In the tests of Table III. this bending always amounted to 90°, and was carried out in a bending-machine driven by a belt. The tests in Table IV. were made by clamping a test-piece between a steam-hammer and its anvil, and hammering the projecting end

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through an angle of about 45°; the steam-hammer was then lifted, the test-piece reversed, and again bent to an angle of about 45° the other way. This process was repeated till the test-piece broke. The number of bends can be looked upon as a fair measure of the ductility of the material. This method had two drawbacks: first, the angle was never properly measured; and secondly, there was great difficulty in always bending the same part of the test-piece. These difficulties were successfully overcome by using an anvil-mould, which was so arranged that the test-pieces could only be bent at one point, and not beyond an angle of 40°, while the radius of curvature was $1\frac{3}{4}$ inch. The last twelve tests of Table IV., and all those contained in Tables V. to VIII., were made with this anvil-mould.

In order to be quite clear, it must be explained that the expression "blue heat" is meant to include all those temperatures which produce discolorations of bright steel or of iron surfaces, ranging from light straw to blue. The temperatures corresponding with the various colours are:—

	Fahrenheit.
Straw colour	470°
Dark straw	480
Violet	490
Blue	500 to 600

In the Tables V. to VIII. the temperatures are pretty accurately defined by these various colours, while in the following remarks the expression "blue heat" will be used to include all these temperatures.

The first question to be considered is: Has a blue heat the same influence on iron as on steel while being bent? The answer is decidedly yes.

It will be noticed that, whereas the lowest number of bends which $\frac{3}{8}$-inch steel would stand at a blue heat was two and a half, one and a half, and two and a half for three different qualities, Lowmoor iron of the same thickness only stood half a bend, or more accurately, 24°, while the thin ($\frac{3}{16}$-inch) iron, only stood three bends when hot, against twenty-one and more when cold.

As iron has been included in these tests, it was necessary to use it in thinner plates than the steel, for it had been found that, whereas some mild steel $\frac{3}{8}$ inch thick would stand from twenty to thirty bends without breaking, Lowmoor iron of the same thickness would break at the third bend. See Table IV.

The next question was suggested by the failure of No. 13,

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Table III. Does bending permanently injure the ductility of steel and iron? This matter has been very exhaustively dealt with in Tables V. and VI. It will be noticed that steel which had been bent cold, either once or twice, would stand almost as many subsequent bends as the original test-pieces. But if the same material was bent once while blue-hot, it lost a great deal of its ductility. Out of the twelve samples in which two preliminary bends were made with the test-piece when heated, nine broke with a single blow of a hammer, and the other three only stood one or two subsequent bends. The same results are to be found in Table VIII. (mild steel). The thin Lowmoor iron does not break quite so easily, but stands about one-half the original number of bends, instead of breaking like glass or cast-iron.

The following Table contains a few extracts and means.

	Medium Hard Steel. $\frac{1}{4}$ inch.	Mild Steel. $\frac{1}{4}$ inch.	Very Mild Steel. $\frac{1}{4}$ inch.	Lowmoor Iron. $\frac{5}{16}$ inch.
Unprepared or annealed	21	12½	26	20
Broken hot (blue)	2½	1½	2½	3
1 preliminary hot bend	3 mean	2½ mean	11 mean	12 mean
2 „ „ bends	½ „	½ „	½ „	10 „
1 preliminary cold bend	20	9½	..	..
2 „ „ bends	19½	8½	19	13
4 „ „ „	..	..	13	11
8 „ „ „	..	..	15	2

Besides subjecting the samples to preliminary bends, some pieces were twisted first one way and then another, both when cold and when hot, in order to ascertain the influence of shearing-stresses. Two twists of 45° in a length of 6 inches do not appear to have affected the ductility (as measured by subsequent cold-bending), no matter whether the test-pieces were twisted when cold or when hot. See Tables V. and VI. Four twists of 90° in lengths of 6 inches had a marked influence, especially on the very mild steel, but less on the thin iron. The number of cold-bends of the steel were reduced from 26 to 15 after cold-twisting, and to 1 after hot-twisting. The number of cold-bends of the thin iron were raised from 20 to 23 after cold-twisting, and lowered to 7 after hot-twisting.

More decided results were obtained with test-pieces which had been slightly thinned down under a steam-hammer. Cold-hammering reduced the ductility from 26 to 21 in the case of the very

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mild steel, and from 20 to 9 in the case of the thin Lowmoor iron. Two samples of the same steel, after being hammered while hot, broke when cold with one hard blow of a hand-hammer (crystalline fracture), while the thin iron stood 6 and 7 bends.

All these results point unmistakably to the great danger which is incurred if iron or steel is worked at a blue heat. The difference between good iron and mild steel seems to be, that iron breaks more easily than steel while being bent when either hot or cold; that iron suffers more permanent injury than steel by cold working, but that if it has successfully withstood bending when hot, there is little probability of its flying to pieces when cold, as is almost sure to be the case with mild steel.

It is a very common practice amongst boilermakers to "take the chill out of a plate" if it requires a little setting, or to set a flanged-plate before it is cold. This is really nothing else than working it at a blue heat, and should not be allowed.

All hammering or bending of iron and steel should be avoided, unless the metals are either cold or red-hot. Where this is impossible, and where the plate or bar has not broken while blue-hot, it should be subsequently annealed.

It is satisfactory to learn that, since the introduction of mild steel, a practice has been gaining ground amongst boilermakers, which must have the effect of guarding against such failures, and should be encouraged. It consists in the cessation of work as soon as a plate, which has been red-hot, becomes so cool that the mark produced by rubbing a hammer-handle or other piece of wood over it will not glow. A plate which is not hot enough to produce this effect, yet too hot to be touched by hand, is most probably blue-hot, and should under no circumstances be hammered or bent.

The theory that local heating of a plate sets up strains which sometimes cause failures, and which is generally accepted as correct, does not appear to have any foundation in fact. It is, however, doubtful whether the proposal to locally reheat a plate, which has been worked hot, in order to anneal this part, should be carried out. This question is dealt with in Tables V. to VIII. Several test-pieces were made red-hot or blue-hot, and were then slowly cooled, by holding one of their edges in cold water. As might have been expected, the medium hard steel lost much of its ductility. The other steels and the iron were not much affected, as will be seen from the following short Table of the number of bends sustained by the samples:—

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——	Medium Hard Steel.	Mild Steel.	Very Mild Steel.	Lowmoor Iron.
Unprepared or annealed	21	12½	26	20
Quenched red-hot in boiling water. .	24	10	..	..
„ „ cold „ . .	1	10	19	20
Red hot, quenched edge in cold water .	3	8	25	27
Blue „ „ „ „ .	3	6½	19, 19	21, 14

Before concluding it will be necessary to revert to the peculiar failure of one of the tensile test-pieces of the medium hard steel, which on being straightened edgeways broke with a crystalline fracture. Of these pieces one was then bent in the anvil-mould, but broke with a partly crystalline fracture at ½ bend instead of standing 21 bends. (See Table V., Nos. 7 and 8.) On inquiry it was found that instead of annealing all the test-pieces of Tables V. and VI. in a plate-heating furnace, they had been baked in a moulder's core-drying oven, and it seems that the temperature of these ovens is about equal to a blue heat. This baking did not at first appear to be an adequate reason for the failure, for all plates which are annealed must pass through a blue heat; but on comparing all the bending-tests of Table V. a peculiarity showed itself, which made it appear probable that the prolonged heating had spoilt the steel. Most of the test-pieces had been made red-hot subsequent to the baking, and either cooled slowly or in water; but a few were never thus heated to redness. It is interesting to compare these with some which had been heated, but otherwise treated in the same way. (Compare Nos. 3, 7, 8, 14, 16, and 20 with Nos. 2, 5, 10, 18, and 19.) The former broke after standing 3, 0, ½, 1, 3 and ½ bends, while the latter stood 21, 22½, 24, 22, and 23 bends. The means are 1½ and 22½ bends. This difference was so glaring that Table VI. was analysed in the same way, but the result was not so marked. The means are respectively 8½ and 10 under the same conditions. Further experiments on this subject will be found in Tables VII. and VIII., Nos. 1 to 12, but the results are as undecided as the last. The averages come to 16 and 20 for iron, and to 22 and 26 for very mild steel.

At first sight it might appear that the medium hard steel plate had by some accident been plunged in cold water while it was still red-hot; but a little reflection will show that, if it had been thus hardened and was so brittle as to break with one blow of a hammer, it could not have been sheared into many small pieces

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without at least one of them breaking. Unfortunately all the test-pieces of this steel have been broken; otherwise the question, whether one quality of steel is and another is not affected by long-continued exposure to a blue heat, might have been investigated. Should it ultimately be shown that the former supposition is correct, then it would be necessary, either to invent some test which would readily detect such steel, or else to take great care not to expose any steel for prolonged periods to a blue heat.

It must be pointed out that such baking may easily occur, if large quantities of iron or steel are annealed in a single furnace. It can also take place in the rolling-mills, especially if there is a deficiency of floor-space. Under such circumstances, it frequently happens that twenty or thirty hot-plates are stacked one upon another. Each plate leaves the rolls at a dull red heat. By the time that the first plate has cooled to a blue heat the next is thrown on it, and maintains the temperature of the bottom one. The third dull red-hot plate is thrown on the others when the second one has also just reached a blue heat. This process is repeated till the pile is complete and blue-hot throughout, at which temperature it remains for a considerable time. If the quality of one of the plates is such as to be affected by this sort of annealing, then there is little doubt that it will have been injured.

It would be of great interest if this question could be further investigated, and also if steel manufacturers could ascertain whether every quality of steel is made permanently brittle by being worked at a blue heat, or whether this is due to the various impurities contained in it.

It should be remembered that iron, though less pure than mild steel, contains the foreign substances in the form of finely-divided slag, and that in good brands the iron layers are probably purer than even the purest mild steel. A very marked difference between iron and steel is the presence of manganese in the latter. The admixture is, of course, necessary on account of the red-shortness of pure Bessemer or Siemens ingot-iron; but it is also possible that blue-shortness (if this expression is permissible) is due to the presence of this substance. On the other hand it must not be forgotten that all tool-steel, which should contain no manganese, is strongly affected in its hardness by the temperatures embraced under a blue heat. This would show that manganese does not influence this quality.

Whatever be the cause of the peculiarity, and whether it will ever be possible to produce steel and iron which are not blue-short,

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it is at least clear that the experiments contained in this Paper show how steel can with certainty be made permanently brittle. In practice everything should therefore be done to avoid any treatments at all involving the working of iron and of steel while blue-hot.

In conclusion, the Author wishes to tender his thanks to the manufacturers who have kindly supplied him with the materials, and to Professor Alex. B. W. Kennedy, M. Inst. C.E., and Messrs. J. Stewart and Son, for the use of their appliances.

The Paper is accompanied by several diagrams, from which the Figs. in the text have been prepared.

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APPENDIX.

TABLE I.—REPEATED TENSILE-TESTS of IRON and HARD and MILD STEEL.

	No.	Materials.	Particulars.	Elastic Limits.	Preliminary.		Ultimate.		
					Stress.	Elongation.	Stress.	Elongation.	Contraction.
				Tons per Sq. inch.	Tons per Sq. inch.	Per cent. in 8 inch	Tons per Sq. inch.	Per cent. in 8 inch.	Per cent.
Cut from one plate.	1	Iron . .	. . . . . C.	16·1	..	..	23·5	12·5	?
	2	„ . .	Elongated twice .	14·8	21·0	4·2	..	..	..
				23·5	..	..	26·0	12·8	?
	3	„ . .	„ four times.	?	?	4·1	..	..	..
			Intervals: 3, 5 and	19·1	22·7	7·2	..	..	..
			21 days . . .	?	?	12·1	..	..	..
				29·1	..	..	30·0	14·2	15·5
	4	Hard steel	Crossways . . C.	19·4	..	..	40·5	17·0	?
	5	„	Lengthways . L.	20·0	..	..	40·5	20·0	?
	6	„	Elongated twice C.	19·3	35·2	5·0	..	..	..
				39·8	..	..	40·3	18·1	?
	7	„	„ „ L.	20·0	35·7	5·0	..	..	..
				43·2	..	..	43·5	19·7	?
	8	„	Quenched red-hot in cold water . .	51·5	..	..	53·5	0	?
	9	„	Tested hot (blue to straw) . . C.	15·9	..	..	51·0	13·0	?
	10	„	Tested hot. L. (straw to pale) . . .	17·0	..	..	51·0	11·0	?
Cut from three plates.	I. 11	Mild steel	Annealed red-hot .	19·3	..	..	29·7	20·1	58·3
	II. 12	„	„ „	16·3	24·6	5·2	..	..	..
			Elongated twice.	28·5	..	..	29·8	17·0	44·0
			Interval: 10 days						
	III. 13	„	Annealed red-hot.	18·4	26·0	0·95	..	..	..
			Elongated twice.	28·0	..	..	28·6	13·0	?
			Interval: 10 days						
	III. 14	„	Annealed red-hot.	16·6	28·8	15·3	..	..	..
			Elongated twice.	32·7	..	..	32·7	19·0	?
			Interval: 15 days						
	II. 15	„	Annealed red-hot.	15·6	23·4	5·4	..	..	..
			Elongated 3 times.	26·9	27·6	7·8	..	..	..
			Intervals: 1 day	29·1	..	..	30·2	14·5	53·3
			and 8 days . .						
	I. 16	„	Quenched red-hot in cold water . .	14·7	..	..	40·0	13·2	43·8
			„ „	9·3	30·2	2·9	..	..	..
	I. 17	„	Elongated 3 times.	33·2	37·0	6·7	..	..	..
			Intervals: 3 & 8 days	38·3	..	..	43·2	10·4	..
	II. 18	„	Elongated while blue	10·0	27·7	4·9	..	..	..
			hot. Re-tested cold	30·7	..	..	33·0	16·0	50·6
	II. 19	„	Elongated while blue	?	?	4·5	..	..	..
			hot. Re-tested twice	30·2	32·3	7·6	..	..	..
			while cold . . .	34·3	..	..	35·6	12·8	43·0
	II. 20	„	Shortened cold under	Shortened		4·7	..	..	..
			steam-hammer. .	11·8	..	..	30·7	7·2	55·0
	II. 21	„	Shortened hot (blue)	Shortened		3·6	..	..	..
			under steam-hammer	24·2	..	..	33·5	8·6	48·6
	I. 22	„	Flattened cold on a	Flattened		1·5	..	..	..
			smith's anvil . .	20·5	..	..	32·0	12·9	65·3
	I. 23	„	Flattened hot (blue)	Flattened		1·5	..	..	..
			on smith's anvil .	25·0	..	..	36·3	9·5	40·5

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TABLE II.—TENSILE TESTS MADE WITH TAPER TEST-PIECES (DIAGRAM) in CONNECTION WITH the BENDING-TESTS of TABLES V. to VIII.

No.	Particulars.	Elastic Limits.	Highest Tensile Strength.		Local Elongation.
		Tons per Sq. inch	Tons per Sq. inch.	Lbs. per Sq. inch.	Per cent.
	Siemens-Martin Steel. Medium hard.				
	35·7 tons = 82,600 lbs.	About			
1	Annealed red-hot	19·6	32·5	72,900	11·54
2	Quenched „ in boiling water	27·2	38·5	86,300	5·83
3[1]	„ „ „ cold „	39·1	48·2	108,000	1·17
4	Annealed „ bent cold to a 15″-radius and flattened	18·3	31·7	71,000	7·22
5	Annealed red-hot; bent hot (violet to straw) and flattened	19·6	33·9	76,000	16·73
	Siemens-Martin Steel. Mild.				
	28·2 tons = 63,100 lbs.				
6	Annealed red-hot	16·1	26·5	59,400	34·72
7	Quenched „ in boiling water	22·3	37·0	82,800	16·46
8	„ „ „ cold „	23·0	36·5	81,900	12·47
9	Annealed „ bent cold to a 15″-radius and flattened	15·6	29·0	65,000	24·26
10	Annealed red-hot; bent hot (blue to dark straw) and flattened	18·3	29·9	67,000	20·16
11	Annealed red-hot; twisted cold (90° in 12″ and back)	?	29·5	66,100	16·83
12	Annealed red-hot; twisted hot (blue to violet) and back	21·2	30·3	67,900	22·44
	Siemens-Martin Steel. Very mild.				
	23·4 tons = 51,400 lbs.				
13	Unprepared	18·3	27·2	61,000	24·80
14	Quenched red-hot in cold water	26·3	39·0	87,400	19·42
15	Annealed blue-hot two nights	19·3	27·4	61,300	20·61
16	„ „ four „	20·1	27·0	60,500	10·40
17	Bent cold to a 15″-radius and flattened	18·5	28·1	63,000	17·98
18	„ hot (blue to straw) „	22·8	29·5	66,000	29·40
19	Twisted cold (90° in 12″ and back)	17·0	28·2	63,100	16·27
20	„ hot (blue to dark straw) „	24·6	31·7	71,000	27·90
	Lowmoor Iron.				
21	Unprepared	12·5	22·1	49,600	6·67
22	Quenched red-hot in cold water	?	24·5	54,800	7·02
23	Annealed blue-hot two nights	13·4	21·7	47,600	18·71
24	„ „ four „	14·1	22·1	49,900	14·16
25	Bent cold to a radius of 15″ and flattened	14·1	23·2	52,100	18·78
26	„ hot (blue to straw) „	12·5	23·6	52,900	7·06
27	Twisted cold (90° in 12″ and back)	15·2	23·2	52,000	8·46
28	„ hot (blue to dark straw) „	16·3	24·6	55,000	16·33

[1] No. 3 broke at a point where the stress was 93,500 lbs. and the elongation 0·38 per cent.

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TABLE III.—VARIOUS TENSILE- and BENDING-TESTS of IRON, and MILD and HARD STEEL.

Particulars.

Defective Steel Garboard Plate, ¾ inch thick.

No.		
1.	Tensile-test 28·8 tons per square inch; 24 per cent. elongation in 8 inches.	
2.	„ 28·9 „ „ 23 „ „ 8 „	
3.	„ between rivet-holes 27·8 tons per square inch, 20 per cent. elongation.	
4.	Tensile-test between rivet-holes annealed 28·8 tons per square inch, 25 per cent. elongation.	
5.	Quenched red-hot in cold water; bent double.	
6.	„ „ „ „ (by machine), 4 angles of . .	90°
7.	Unprepared „ „ 2 „ . .	90°
8.	Annealed „ „ 4 „ . .	90°
9.	Punched before shearing; broke without bending	0°
10.	„ after „ „ „	0°
	Other end made red-hot bent to right-angle through punched hole; cooled slowly and bent double	90°
11.	Punched before shearing, then annealed; bent (machine)	90°
12.	„ „ „ „ „	100°
	Other end bent while blue-hot, broke through rivet-hole	45°
13.	Punched after shearing and annealing; not broken	100°
	Other end bent to 30° while blue-hot; cooled slowly for an hour, broke with one blow of a hammer	0°
14.	Bent through original rivet-holes; result good.	
15.	„ „ „ „	

Mild Siemens-Martin Steel, ¾ inch thick.

16.	Tensile-test 25 tons per square inch; 22 per cent. elongation.	
17.	Quenched red-hot in cold water; bent double (steam-hammer) . .	180°
18.	„ „ „ „ (machine), 6 angles of . . .	90°
19.	„ „ „ „ „ 6 „ . . .	90°
20.	Annealed red-hot . . . . „ „ 5 „ . . .	90°
21.	Bent blue-hot . . . . . broke	30°
22.	„ „ to an angle of 15° when cold; „	90°

Hard Siemens-Martin Steel, 7/16 inch thick.

23.	Tensile-test 37 tons per square inch; 20 per cent. elongation.	
24.	Annealed red-hot bent (machine) 6 angles . .	90°
25.	Quenched red-hot in cold water; „ „ 2 „ . . .	90°
26.	Annealed blue-hot „ „ cold, 2½ „ . . .	90°
27.	Bent blue-hot „ „ hot 2½ „ . . .	90°

Hard Spring Steel, ¾ inch thick.

28.	Annealed red-hot; bent (machine) 2½ angles	90°
29.	„ blue „ „ „ cold, 1½ „	90°
30.	Bent „ „ „ „ hot 2½ „	90°
31.	„ straw „ „ „ „ 2½ „	90°

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TABLE III.—*continued.*

Iron equal to Lowmoor, $\frac{7}{10}$ inch thick.

No.				
32.	Unprepared	bent (machine)	2 angles	90°
33.	„	„ „	2 „	45°
34.	Quenched red-hot in cold water	„ „	cold, 4 „	45°
35.	Bent blue-hot	„ „	hot 2 „	45°
36.	„ straw „	„ „	„ 2 „	45°

No.			Breaking-stress.	Elongation.
37.	Tensile test of steel, cold	L[1]	28·1 tons.	23 per cent.
38.	„ „ „ „	C	29·6 „	23 „
39.	„ „ „ blue-hot, straw fracture	L	36·2 „	15 „
40.	„ „ „ „ „ „	C	35·3 „	15½ „
41.	„ „ „ dark red, blue „	L	27·8 „	21 „
42.	„ „ „ „ black „	C	18·4 „	25 „
43.	„ „ best iron, cold	L	22·2 „	24 „
44.	„ „ „ „	C	23·0 „	20½ „
45.	„ „ „ blue-hot, straw fracture	L	31·1 „	16½ „
46.	„ „ „ „ „	C	29·2 „	10 „

TABLE IV.—BENDING-TESTS of VARIOUS STEELS and IRONS.

The bending was done with a sledge-hammer. The bends were approximately equal to 45°.

Particulars.

Mild Steel, $\frac{3}{8}$ inch thick.

No.		No. of bends.
1.	Unprepared	10
2.	Annealed red-hot	21
3.	Quenched „ in boiling water	14
4.	„ „ tepid „ (82° Fahrenheit)	6
5.	„ „ ice-cold „	2
6.	Red hot cooled slowly to straw, bent hot	5
7.	Bent hot (pigeon grey)	6
8.	„ (blue)	6
9.	„ (violet)	2
10.	„ (straw)	4
11.	Bent twice while hot (blue) quenched in water, bent cold[2]	2½
12.	„ „ (straw), cooled slowly „	1

Mild Steel, $\frac{3}{8}$ inch thick.

13.	Unprepared	16
14.	Quenched red-hot in cold water	16
15.	Bent hot (pigeon grey), fracture straw	4
16.	„ (blue), fracture dark straw	2½
17.	Unprepared, bent double.	
18.	Quenched red-hot in cold water, bent double.	

[1] L and C signify Lengthways and Crossways.

[2] Broke short 1 inch from original bend.

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TABLE IV.—*continued.*

Mild Steel, ¾ inch thick.

No.		
19.	Annealed red-hot	10½
20.	„ dark red-hot	10
21.	Quenched red-hot in cold water	10
22.	Bent hot (blue)	2½
23.	„ (violet)	2½
24.	Bent twice while hot (blue), cooled slowly (bending reversed)	1½
25.	„ „ „ („ continued)	2½
26.	Unprepared	6½
27.	Annealed red-hot	10½
28.	Quenched red-hot in cold water	10½
29.	Unprepared, bent twice; after 2 weeks broke	5
30.	„ „ „ „ 6 „	2
31.	„ „ IV.; „ 6 „	1
32.	„ „ „ „ 6 „	4
33.	Annealed red-hot, bent IV.; after 6 weeks broke	9
34.	Quenched „ „ II.; „ 6 „	4
35.	„ „ „ IV.; „ 6 „	7
36.	Bent hot (dark straw)	1½

Basic Steel, mild quality, ¾ inch thick.

37.	Unprepared	16
38.	Quenched red-hot in cold water	16
39.	Bent hot (blue), fracture straw	4
40.	„ „ „ dark straw	2½

Basic Steel, hard quality, ¾ inch thick.

41.	Unprepared	12
42.	Quenched red-hot in cold water	1
43.	Bent hot (blue), fracture dark straw	4

Lowmoor Iron, ¾ inch thick.

Bent in anvil mould to 40° with a radius of 1¾ inch.

		Cracked.	Broke.
44.	Lengthways, unprepared	2	3 (n.b.)
45.	„ „	2	3 „
46.	„ bent hot (blue), broke with 1 hard blow	..	0
47.	„ „ (straw) „ 1 „	..	0
48.	„ „ (blue), light hammering	26°	.. (n.b.)
49.	„ „ (straw) „	37°	.. „
50.	Crossways, unprepared	2½	3 „
51.	„ „	2½	3 „
52.	„ bent hot (blue), light hammering	33°	.. „
53.	„ „ (violet) „	33	.. „
54.	„ „ (straw) „	29	.. „
55.	„ „ (light straw) „	24	.. „

NOTE.—(n.b.) signifies that the test-piece did not break in two.

The Roman numerals denote the number of preliminary bends.

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TABLE V.—BENDING-TESTS of SIEMENS-MARTIN STEEL. MEDIUM HARD, 35·7 TONS PER SQUARE INCH.

See Table II. and Diagrams for Tensile-Tests.

Particulars.

Dimensions of Test-pieces, 6 inches × 1½ inch × 0·34 inch.

Sheared from one plate.

Every recorded bend amounts to 40°, curvature 1¾-inch radius.
Every test-piece was baked in a moulder's core-drying oven for two nights.

No.		No. of bends. Cracked.	Broke.	
	Annealed at a red or blue heat.			
1.	6 D.[1] Unprepared except by baking	..	15	
2.	6 D. Annealed red-hot	17	21	
3.	6 D. „ violet	1	3	
4.	6 D. „ „ 35 D.	3	5	
5.	6 D. „ red hot; 5 D. Annealed blue	19	22½	
6.	5 D. Quenched red-hot in boiling water; 1 D. Annealed violet; 8 D.	17	23	(n.b.)
7.	1 D. Bent edgeways; broke without bending	..	0	
8.	1 D. „ flatways	..	½	
	Red- or blue-hot quenched in water.			
9.	5 D. Quenched red in cold water	..	1	(n.b.)
10.	5 D. „ „ boiling water; 1 D.	23	24	
11.	5 D. „ „ „ „ quenched straw in cold water; 1 D.	16	17	
12.	5 D. Quenched red in boiling water; quenched straw in cold water; 1 D.	..	26	
13.	5 D. Quenched red in boiling water; quenched straw in cold water; 6 D.	18	19	
14.	6 D. Quenched violet in cold water; 7 D.	..	1	
	Edges quenched in cold water.			
15.	6 D. Quenched dark straw in cold water; 35 D. Red-hot, quenched edge in cold water	1	3	
16.	6 D. Quenched violet in cold water; 35 D. Blue-hot, quenched edge in cold water	1	3	
	Broken while blue-hot.			
17.	6 D. Blue-hot; fracture straw	..	2½	
	BENT COLD AFTER PRELIMINARY TWISTING.			
	Twist in 6 inches, 45° one way and back.			
18.	6 D. Annealed red-hot, twisted cold; 7 D.	19	22	
19.	6 D. „ „ quenched red in boiling water, twisted cold; 7 D.	17	23	(n.b.)
20.	6 D. Annealed blue, twisted cold; 7 D.	..	½	
21.	6 D. „ red-hot, twisted hot (blue); 7 D.	17	21	

[1] D attached to a numeral denotes an interval of a certain number of days; (n.b.) signifies that the test-piece did not break in two at the last bend; the Roman numerals I. and II. denote one or two preliminary bends.

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TABLE V.—*continued.*

BENT COLD AFTER PRELIMINARY BENDING.

No.		No. of bends. Cracked.	Broke.
	One preliminary bend.		
22.	6 D. Annealed red-hot, I. cold bend; 7 D.	14	20
23.	5 D. „ „ 1 D. I. cold bend; 35 D. . . .	18	20 (n.b.)
24.	5 D. Quenched red-hot in boiling water; 35 D. I. cold bend; 1 D.	21	25 (n.b.)
25.	5 D. Quenched red-hot in boiling water; 1 D. I. cold bend; 7 D.	10	16 (n.b.)
26.	5 D. Quenched red-hot in boiling water; 1 D. I. cold bend; 35 D.	12	16 (n.b.)
27.	5 D. I. hot bend (dark straw); 1 D.	5	5½
28.	6 D. „ „ (light straw); 7 D.	1	2
29.	6 D. „ „ (blue); 35 D.	..	1½
	Two preliminary bends.		
30.	6 D. Annealed red-hot; 5 D. II. cold bends; 1 D. Bending continued	15	18½
31.	6 D. Annealed red-hot; II. cold bends; 7 D. Bending continued	17	21
32.	5 D. Annealed red-hot; II. cold bends; 35 D. Bending continued	17	20 (n.b.)
33.	5 D. Annealed red-hot; 5 D. II. cold bends; 1 D. Bending reversed	13	17 (n.b.)
34.	5 D. Annealed red-hot; II. cold bends; 7 D. Bending reversed	15	19
35.	5 D. Annealed red-hot; II. cold bends; 35 D. Bending reversed	17	21 (n.b.)
36.	5 D. Quenched red-hot in boiling water; 35 D. II. cold bends; 1 D. Bending continued	15	19
37.	5 D. Quenched red-hot in boiling water; 1 D. II. cold bends; 7 D. Bending continued	15	21 (n.b.)
38.	5 D. Quenched red-hot in boiling water; 1 D. II. cold bends; 35 D. Bending continued	18	22
39.	5 D. Quenched red-hot in boiling water; 35 D. II. cold bends; 1 D. Bending reversed	17	20
40.	5 D. Quenched red-hot in boiling water; 1 D. II. cold bends; 7 D. Bending reversed	13	15
41.	5 D. Quenched red-hot in boiling water; 1 D. II. cold bends; 35 D. Bending reversed	..	17½
42.	5 D. II. hot bends (straw); 1 D. Bending continued	..	1
43.	5 D. „ „ (light straw); 7 D. „ „	..	0
44.	6 D. „ „ (dark straw); 35 D. „ „	..	1
45.	5 D. „ „ (light straw); 1 D. „ reversed	..	0
46.	6 D. „ „ (straw); 7 D. „ „	..	0
47.	6 D. „ „ (dark straw); 35 D. „ „	..	0

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TABLE VI.—BENDING-TESTS OF SIEMENS-MARTIN STEEL. MILD, 28·2 TONS PER SQUARE INCH.

See Table II. and diagrams for tensile-tests.

Particulars.

Dimensions of Test-pieces, 6 inches × 1½ inch × 0·35 inch.

Sheared from one plate.

Every recorded bend amounts to 40°, curvature 1¾-inch radius.
Every test-piece was baked in a moulder's core-drying oven for two nights.

No.		No. of bends. Cracked.	Broke.
	Annealed at a red or blue heat.		
1.	6 D. Unprepared except by baking	3	6
2.	6 D. Annealed red-hot	9	12½
3.	6 D. „ violet	3	8
4.	6 D. „ „ 35 D.	3	7½
5.	6 D. „ red-hot; 35 D. Annealed straw; 1 D.	3	7 (n.b.)
6.	5 D. Quenched red-hot in cold water; 1 D. Annealed violet; 7 D.	7	9
	Red- or blue-hot quenched in water.		
7.	5 D. Quenched red in cold water; 1 D.	7	10
8.	5 D. Annealed red-hot; 35 D. boiling water	..	10
9.	5 D. „ „ cold water; 1 D. Quenched straw in cold water; 1 D.	..	10
10.	5 D. Quenched red in cold water; 1 D. Quenched dark straw in cold water; 7 D.	7	9 (n.b.)
11.	5 D. Quenched red in cold water; Quenched straw in cold water; 36 D.	12	12½
12.	6 D. Quenched violet in cold water	3	6
	Edges quenched in cold water.		
13.	6 D. Quenched dark straw in cold water; 35 D. Red hot, quenched edge in cold water	5	8 (n.b.)
14.	6 D. Quenched violet in cold water; 35 D. violet, quenched edge in cold water	3	6½ (n.b.)
	Broken while blue hot.		
15.	6 D. Blue-hot; fracture light blue	..	1½
	BENT COLD AFTER PRELIMINARY TWISTING.		
	Twists in 6 inches, 45° one way and back.		
16.	5 D. Annealed red-hot; 1 D. Twisted cold; 7 D.	3	11
17.	5 D. „ „ 1 D. Quenched red in cold water, twisted cold; 7 D.	5	7
18.	6 D. Annealed violet, twisted cold; 7 D.	9	11
19.	5 D. „ red-hot; 1 D. Twisted hot (blue); 7 D.	5	9

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TABLE VI.—*continued.*

BENT COLD AFTER PRELIMINARY BENDING.

No.		No. of bends. Cracked.	Broken.
	One preliminary bend.		
20.	5 D. Annealed red-hot; 1 D. I. cold bend; 7 D.	..	10
21.	5 D. „ „ „ „ „ 35 D.	..	9
22.	5 D. Quenched red-hot in cold water; 6 D. I. cold bend; 1 D.	1	4
23.	5 D. Quenched red-hot in cold water; 1 D. I. cold bend; 7 D.	4	6
24.	5 D. Quenched red-hot in cold water; 1 D. I. cold bend; 35 D.	..	5½
25.	5 D. I. hot bend (grey cooled to blue); 1 D.	..	1
26.	5 D. „ „ (straw); 7 D.	3	4
27.	6 D. „ „ (light straw); 35 D.	1	1½
	Two preliminary bends.		
28.	5 D. Annealed red-hot; 35 D. II. cold bends; 1 D. Bending continued	7	9 (n.b.)
29.	5 D. Annealed red-hot; 1 D. II. cold bends; 7 D. Bending continued	7	9
30.	5 D. Annealed red-hot; 1 D. II. cold bends; 35 D. Bending continued	..	4½ (n.b.)
31.	6 D. Annealed red-hot; 35 D. II. cold bends; 1 D. Bending reversed	3	7
32.	5 D. Annealed red-hot; 1 D. II. cold bends; 7 D. Bending reversed	7	11
33.	6 D. Annealed red-hot, II. cold bends; 35 D. Bending reversed	7	9½
34.	5 D. Quenched red in cold water; 35 D. II. cold bends; 1 D. Bending continued	1	5
35.	5 D. Quenched red in cold water; 1 D. II. cold bends; 7 D. Bending continued	..	7 (n.b.)
36.	5 D. Quenched red in cold water; 1 D. II. cold bends; 35 D. Bending continued	..	4½
37.	5 D. Quenched red in cold water; 6 D. II. cold bends; 1 D. Bending reversed	3	5½
38.	5 D. Quenched red in cold water; 1 D. II. cold bends; 7 D. Bending reversed	7	12
39.	5 D. Quenched red in cold water; 1 D. II. cold bends; 35 D. Bending reversed	3	5
40.	5 D. II. hot bends (blue); 1 D. Bending continued	..	0
41.	6 D. „ „ (dark straw); 7 D. „ „	..	0
42.	6 D. „ „ (violet); 35 D. „ „	..	0
43.	5 D. „ „ (dark straw); 1 D. „ reversed	..	2
44.	6 D. „ „ (dark straw); 7 D. „ „	..	0
45.	6 D. „ „ (straw); 35 D. „ „	..	0

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TABLE VII.—BENDING-TESTS of LOWMOOR IRON.

See Table II. and diagrams for tensile-tests.

Particulars.

Dimensions of test-pieces: 6 inches × 1½ inch × 0·19 inch.

Sheared from one plate.

Every recorded bend amounted to 40°. Curvature 1¾ inch radius.

No.		Number of bends. Cracked.	Broke.
	Annealed at a red and blue heat.		
1.	Unprepared	13	21 (n.b.)
2.	Annealed red-hot	..	19
3.	„ blue; 7 D.	6	15 (n.b.)
4.	„ „ 7 D.	11	19 „
5.	„ violet; 7 D.	11	14 „
6.	Baked for 2 nights; 5 D.	9	18 „
7.	„ 4 „	7	15 „
8.	„ 8 „	9	17 „
9.	„ 4 „ 3 D. Annealed dark straw; 7 D.	9	15 „
10.	„ 8 „ Annealed blue; 3 D.	11	15 „
11.	„ 13 „ Straw to violet; 4 D.	7	18 „
12.	Quenched red in cold water; 4 D. Baked one month; 28 D.	23	29
13.	„ blue „ 4 D. „ „ 28 D.	6	15
	Red- and blue-hot, quenched in cold water.		
14.	Quenched red-hot; 4 D.	17	20
15.	„ violet; 7 D.	7	17 (n.b.)
16.	Red-hot, quenched edge; 4 D.	20	27 „
17.	Pigeon-grey „ 3 D.	11	21 „
18.	Violet „ 3 D.	7	14 „
	Broken while blue-hot.		
19.	Blue-hot; fracture blue	..	5 (n.b.)
20.	„ „ straw	..	4½
21.	Violet; „ dark straw	..	3
22.	Dark straw; „ light „	..	5
23.	Quenched red in cold water; 4 D. violet; fracture straw	..	7
	Flattened hot and cold under a steam hammer.		
24.	Reduced 16 per cent. cold; 3 D.	7	9 (n.b.)
25.	„ 12 „ hot (blue); 3 D.	3	6 „
26.	„ 6 „ „ (violet); 3 D.	..	7
	BENT COLD AFTER PRELIMINARY TWISTING.		
	Four twists of 90° in a length of 6 inches.		
27.	Quenched red in cold water; 4 D. twisted cold	19	23 (n.b.)
28.	„ violet „ 4 D. „	..	15 „
29.	Twisted hot (blue to light straw)	..	7 „

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TABLE VII.—*continued.*

BENT COLD AFTER PRELIMINARY BENDING.

No.		Number of bends. Cracked.	Broke.
	One preliminary hot bend.		
30.	I. Hot bend (blue); 7 D.	12	16 (n.b.)
31.	I. „ (dark straw); 7 D.	8	11 „
32.	I. „ (light „); 7 D.	4	10 „
	Two preliminary hot bends.		
33.	II. Hot bends (blue); 7 D.	6	10½ (n.b.)
34.	II. „ (dark straw); 7 D.	7	12½ „
35.	II. „ (light „); 7 D.	3	6 „
	Several preliminary cold bends.		
36.	II. Cold bends, annealed violet; 4 D.	6	13 (n.b.)
37.	II. „ quenched blue in cold water 4 D. . . .	8	13 „
38.	Quenched red in cold water; 4 D. IV. Cold bends; 3 D.	..	11 „
39.	VIII. Cold bends, annealed blue	4	7 „
40.	VIII. „ quenched dark straw in cold water; 4 D.	4	7 „
41.	VIII. „ 50 D.	1	2 „
42.	Annealed blue; 7 D. VIII. Cold bends; 50 D. . . .	3	6 „

TABLE VIII.—BENDING-TESTS of SIEMENS-MARTIN STEEL. VERY MILD, 23·4 TONS PER SQUARE INCH.

See Table II. and diagrams for tensile tests.

Particulars.

Dimensions of test-pieces: 6 inches × 1½ inch × 0·40 inch.

Sheared from one plate.

Every recorded bend amounted to 40°. Curvature 1¾ inch radius.

No.		Number of bends. Cracked.	Broke.
	Annealed at a red and blue heat.		
1.	Unprepared	19	27 (n.b.)
2.	Annealed red-hot	19	25
3.	„ blue; 7 D.	15	19 (n.b.)
4.	„ violet; 7 D.	20	24 „
5.	„ dark straw; 7 D.	11	27 „
6.	„ „ 7 D.	14	18 „
7.	Baked for 2 nights; 5 D.	13	23 „
8.	„ 4 „	7	18
9.	„ 8 „	15	21 (n.b.)
10.	„ 4 „ 3 D. Annealed straw; 7 D. . . .	23	25 „

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TABLE VIII.—*continued.*

No.		Number of bends. Cracked.	Broke.
11.	Baked for 8 nights. Annealed dark straw; 2 D. . . .	19	22 (n.b.)
12.	„ 14 „ (Straw to blue)	13	20 „
13.	Quenched red in cold water; 4 D. Baked one month; 28 D.	20	33 „
14.	„ violet „ 4 D. „ „ 28 D.	6	11 „
	Red- and blue-hot, quenched in cold water.		
15.	Quenched red-hot; 4 D.	11	19 (n.b.)
16.	„ „	15	20
17.	„ blue; 6 D.	..	10 (n.b.)
18.	Red-hot, quenched edge; 4 D.	18	25 „
19.	Blue „ „ 2 D.	14	19 „
20.	Violet „ „ 2 D.	15	19 „
	Broken while blue-hot.		
21.	Blue; fracture straw	..	5
22.	„	..	4
23.	Violet	..	2½
24.	Light straw; fracture very light straw	..	7
25.	Quenched red in cold water; 4 D. violet	..	4
	Flattened hot and cold under a steam-hammer.		
26.	Reduced 12 per cent. cold; 2 D.	15	21 (n.b.)
27.	„ 18 „ hot (blue); 2 D.	..	0
28.	„ 12 „ „ (violet); 2 D.	6	0
	BENT COLD AFTER PRELIMINARY TWISTING.		
	Four twists of 90° in a length of 6 inches.		
29.	Quenched red in cold water; 4 D. twisted cold	10	15 (n.b.)
30.	„ violet „ 4 D. „	14	18 „
31.	Twisted hot (blue to straw); 2 D.	..	1
	BENT COLD AFTER PRELIMINARY BENDING.		
	One preliminary hot bend.		
32.	I. Hot bend (blue); 7 D.	9	13 (n.b.)
33.	I. „ (dark straw); 7 D.	9	14 „
34.	I. „ (light „); 7 D.	3	6 „
	Two preliminary hot bends.		
35.	II. Hot bends (blue); 7 D.	..	2
36.	II. „ (dark straw); 7 D.	..	0
37.	II. „ (light „); 7 D.	..	0

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TABLE VIII.—*continued.*

Several preliminary cold bends.

No.		No. of bends. Cracked.	Broke.
38.	II. Cold bends, annealed violet; 4 D.	17	19 (n.b.)
39.	II. ,, quenched violet in cold water; 4 D. . .	9	11 ,,
40.	Quenched red in cold water; 4 D. IV. Cold bends; 3 D.	10	13 ,,
41.	VIII. Cold bends, annealed blue; 4 D.	2	9 ,,
42.	VIII. ,, quenched violet in cold water; 4 D. . .	1	1½
43.	VIII. ,, 50 D.	7	15
44.	Annealed blue; 7 D. VIII. Cold bends; 50 D.	..	0

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Discussion.

Sir Frederick Bramwell.

Sir FREDERICK BRAMWELL, President, in proposing a vote of thanks to Mr. Stromeyer, said that the Paper dealt with a subject of the very highest importance. Year by year mild steel was being substituted for wrought iron, and no doubt with very beneficial effects, whether in regard to its employment for ship-building, bridge-building, boiler-making, or in the construction of artillery. Nevertheless there were occasional instances of failure which excited alarm, and he thought that the Institution could not be better occupied for one or two evenings in considering with the Author whether it was possible to discover what were the causes commonly regarded as obscure which had led to the few instances of failure that had been recorded.

Mr. Stromeyer.

Mr. STROMEYER wished, before the discussion commenced, to draw attention to the stress diagrams, especially Figs. 6 and 7, as the influence of various preliminary operations was well shown in them. Thus it would be seen that the unprepared and quenched, and the cold bent and twisted pieces gave concave curves, while the others, which were either baked, bent or twisted at a blue heat, gave curves which were almost straight. There was also a considerable difference shown in Fig. 4, the material being Low Moor iron. Four specimens had been treated at a blue heat, and four cold. He could not give any reason for these differences; but he had published the tests, so that any persons making experiments might be able to compare the qualities. He also wished to refer to a tensile-test made with a piece of steel that had been drawn out blue-hot. Previously to the above experiments, he had stretched an ordinary test-piece after it had been flattened and thereby elongated at a blue heat, and the result was shown in Table I., No. 22. The steel from which this piece had been cut had been tested after annealing, and found to have (see No. 11) an elastic limit of 19 tons, and an ultimate breaking-stress of 29·7 tons, with an elongation of 20 per cent. In the flattened piece the limit rose to 20 tons, the breaking stress to 32 tons, and the elongation was reduced nearly one half, viz., 12·9 per cent. This showed that although the metal was made stronger by flattening it at a blue heat, its ductility was decidedly reduced. He exhibited a test-piece which had been prepared in order to show the effect of working steel at a blue heat. It was $\frac{1}{2}$-inch thick, and sheared wide about 2 inches. One end was bent nearly double when cold. The other end was put between two

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pieces of red-hot iron and when blue-hot was flattened under a steam-hammer to about two-thirds of its original thickness. After an interval of a day it broke with one heavy blow of a hammer, and the fracture, as would be seen, was perfectly crystalline. Another piece, but of smaller dimensions, had been prepared in the same way by being drawn out at a blue heat at one end, while the other one had been bent double to show the ductility of the metal. He then broke this piece by striking it on an anvil, and pointed out that it had broken at a point where the heat had not exceeded a straw colour. This peculiarity of steel might perhaps explain how bayonets, about which a good deal had recently been said, had been spoilt. It was possible that after they were drawn out at a red heat the finishing was done while they were blue-hot. Some friends had suggested that if the working of steel or even iron at blue heat spoiled it permanently, rivets of any sort would not be able to stand, for although they were red-hot to begin with they must pass through a blue heat before cold. It was a fact, however, that riveting did stand, and his explanation was that, in all riveting, especially in piecework, the riveters after hammering a rivet at red heat left off work for a time, while engaged in hammering the next one, and sometimes two, when this was done they would go back and re-rivet (harden up) the first one. Thus they left off while the rivet was going through the temperature of the blue heat. It was red to begin with, and the first part of the hammering was done then; as it cooled to blue it was in a dangerous condition, and the hammering was stopped; then, when it became cold enough to be hammered again, the work was completed. He thought that if the rivets were worked continuously from a red heat till they were quite cold they would often break. He noticed in the case of ships that had been in collision, and where it was necessary to cut out many rivets, that three or four out of a dozen would always appear with a crystalline fracture. This, in his opinion, was due to the hammering having been continued too long or recommenced too soon. He did not suppose that the workmen understood the question of the blue heat, but they had evidently found out by practice that if they continued to hammer until the rivet was cold the result was unsatisfactory, and that they had to do it over again. Mr. Stromeyer.

Mr. Percy C. Gilchrist observed that the members would be still more indebted to the Author if he would kindly supplement the Paper by analyses of the samples upon which he had operated. It was a satisfaction to know that, for the first time, iron and steel Mr. Gilchrist.

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Mr. Gilchrist. appeared to be in the same boat, both being injuriously affected by a blue heat. Reference was made in the Paper to a steel plate which cracked while it was being straightened, due, he thought, to too much reliance being attached to the quench test, a test that was excellent for quickly ascertaining if the sample operated upon contained too much silicon, phosphorus, manganese, and carbon, but which supplied no information of the physical condition of the material operated upon. If a plate had been produced from an ingot or bloom, one end of which had been burnt and which had not received sufficient work to destroy the ill-effects so produced, or if the plate had been injured in the rolling, one end perhaps having been rolled too cold; the quench test would then show the plate to be ductile; and yet if a piece were tested straight from the shears it would break without bending, the discrepancy between the two tests being due to the fact that, in quenching, the test-piece had been practically annealed, for making it red-hot annealed it, and the subsequent quenching in cold water did not affect the bending properties of good soft steel. In order, therefore, to rapidly tell whether the steel was right both chemically and physically, he suggested that pieces from the plate representing each end of the ingot or bloom should be tested, both after quenching and also direct from the shears. In this manner it would be shown not only whether the steel was right chemically, but also whether it was left in its softest physical state. When the quench test stood better, as it often did, when the test came straight from the shears, it was not in its softest physical state, and should be annealed and retested, and if the tests did not then agree the plate should not be used. The spoiling referred to by the Author when bending at a blue heat appeared to him to be analogous to that caused by stamping steel. Thus, suppose a piece of steel were punched into a bowl, and were then left until the following day, the bowl would not submit to be stamped deeper without working; that was the steel had been made physically hard; if, however, it were annealed and brought into a soft state, say with all its particles at rest, then it could be stamped still deeper. Probably if the steel that had been subjected to a blue heat were afterwards annealed it also would be restored to its original ductility. As to the use of the word "mystery" in connection with failures in steel, he thought this was but a polite term to express our ignorance as to the best and proper way of treating steel. There was no doubt that although mild steel surpassed the best iron in possessing equal strength lengthways and crossways, and also in possessing greater elonga-

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tion and contraction of area than the best iron, yet that it required somewhat different treatment to iron. Nevertheless the lower the tensile-strain possessed by steel the more nearly did it approach the best Yorkshire iron, and the more abuse it would stand, such as cold rolling, punching, shearing, heating it unequally, &c., without being spoilt. Thus, if a steel were taken containing carbon 0·05 per cent., manganese 0·3 per cent., phosphorus 0·05 per cent., with 23 or 24 tons tensile strain, it would also have 25 to 30 per cent. of elongation, 55 to 65 per cent. contraction of area, and the same strength longitudinally and transversely, and would be spoilt with difficulty. If engineers would keep to low tensile strains he felt sure that the material would be less easily spoilt than harder steels, and that there would be fewer so-called mysteries to be accounted for. Mr. Gilchrist.

Mr. E. H. CARBUTT, M.P., said that although he was an engineer and an Associate of the Institution, he had not followed his profession for the last few years, and therefore his education was not up to the present day. He had come, however, to learn something about a question which had that day been discussed in the House of Commons—the question of defective bayonets. There had been a great outcry in the country because the bayonets supplied to the army had failed, and during the Egyptian war especially had proved very defective. It had been proved, he believed, satisfactorily, that one-third of the bayonets supplied to three regiments had proved defective. That was a very serious matter, because if one-third of the soldiers found their implements defective, they would lose confidence, and not exhibit the acts of gallantry to which Englishmen had been accustomed. He should be glad, therefore, if the President, who was a member of the Ordnance Committee, could tell the meeting something about the question of annealing steel, which perhaps, as the Author had suggested, might have something to do with the grievance complained of. It appeared that some of the bayonets had been made from German steel, and some from steel supplied by one or two leading firms in Sheffield. The Government had promised that a report should be presented upon the question. He greatly disapproved of the way in which English arsenals were conducted; and he hoped a change would soon be effected in their management in a right direction. With reference to the annealing of steel, he might mention that on lately visiting Woolwich he noticed the process of shrinking the hoops on the guns. They were warmed by being put into a fire made of pieces of wood, in the open air. His own opinion was, Mr. Carbutt.

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Mr. Carbutt. that the hoops ought to be shrunk on with a certain tension, and at a proper temperature. It seemed a barbarous method to heat them out of doors by pieces of wood at an unknown temperature, and then to shrink them on. Surely the largest manufacturing nation in the world ought to be able to do better than that.

Mr. Reynolds. Mr. EDWARD REYNOLDS thought that the peculiar behaviour of material to which reference had been made was wholly a question of quality, and ought not to be accepted as indicating a normal and invariable condition. With regard to bayonets, it might be taken for granted that, whether or not in the course of manufacture they had been worked at a blue heat, they would certainly afterwards have been made red-hot, which would remove any special molecular condition arising therefrom; they would have been hardened in water, and then brought purposely to a blue heat, giving the spring temper which was the most trustworthy condition known. Many of the tests reported showed that the material was soft. That again might be a question of quality of material, or of what steel makers called right temper, which meant carbonization, not what the scientific world called temper; but it was equally likely to have resulted from the notorious habit soldiers had of using their bayonets as pokers, &c. Another example of working at a blue heat occurred in file hardening, which he had often watched in file makers' works. In the ordinary way, when a file was hardened, it was brought to a uniform red-heat by being immersed in red-hot melted lead; it was then dipped in water, probably with some solution in it, and it was cooled to just about "blue heat" and taken out again, being so hard that the teeth were not easily damaged. If warped in hardening, as frequently happened, a man with a lever prized it under a bar, and then hammered it on a lead or wooden block with a heavy mallet. He then dipped it again, and the hot file made a decided fizzing in the water. If the simple fact of working at a blue heat was fatal, that would be impossible; and yet it was done thousands of times every day. He had seen such heavy blows given, that he believed the file could not possibly stand them if it were cold. He did not make these statements in order to controvert facts which were indisputable. The authorities who brought such matters forward never did so lightly, but always with the greatest care. The conclusion which he wished to draw was, that far too much was expected from the material, especially in relation to its price. During the last twenty-five years he had had very little to do with what might

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be called inferior or common materials; but that day before leaving his works he thought he would try how the materials at hand would behave. He first took three pieces of Swedish bar-iron 1 inch in diameter; they were brought out of a store where they were exposed to frost. One piece was bent cold; another piece was ground bright and heated until it was deep orange, and then bent; a third piece was heated red-hot, as he thought the question might arise whether the heating up or the cooling down affected the question. All the three pieces were bent close with a steam-hammer, so that the doubled bar measured rather less than twice the original diameter. The first specimen of Swedish iron, after being heated and cooled, did start a pile seam, a little of it peeled up; he therefore took another, which bent close without any such result. The next was a piece of steel such as was used for rivets for girders, of about 28 tons to the inch tensile-strength. The steel had been treated in a reeling-mill, which was a mill for rolling in a circular direction between rolls, so as to make the bar circular, it being intended for cold riveting. The steel here was always rolled until it was practically black. The result was that there was no apparent difference in behaviour between the piece taken out of the store exposed to frost, after being finished at a supposed blue heat, and those specimens heated up and cooled down, except that the men thought that the piece heated up bent rather more easily. He repeated the experiment with some very mild steel, and the result was the same. Of course, if a number of flexures had been tried, it was possible that a difference might have been found, but no one ever practically wanted iron or steel to do more than bend double without any sign of distress. With regard to the particular heat in question, about forty years ago there were published some experiments by the late Mr. (afterwards Sir William) Fairbairn with reference to the supposed effect produced on the strength of the boiler-plates by the heat of the steam in the boiler. The plate had the highest tensile strength at about 600°; and although the experiments were made without analyses, it was singular that the heat at which the iron was the strongest, and to which steel had to be raised in tempering to bring it to the spring temper, was this blue heat; it was certainly very strange if it was necessarily dangerous in other things. Mr. Reynolds.

Mr. H. H. West doubted whether the blue heat was the critical condition that it was generally supposed to be. He did not know that he could give any good reason for his opinion, but possibly the conditions which were really critical were masked under the Mr. West.

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Mr. West. blue heat, something happening of which engineers were not cognizant. He had thought that there might be a sort of re-crystallization, and that it occurred at or about the heat in question, when the ultimate molecules of the steel were in a state of want of cohesion among themselves. If a little fusible metal was allowed to cool until it was nearly solid, it became exceedingly friable, and broke up into almost a powder, and came into a sort of pasty granular condition. Possibly a somewhat analogous condition occurred in steel at or about that temperature, and if in that state it received any violence, a disturbance of the cohesion of the particles might not reach the surface and be palpable to the eye or to any physical test, and yet, as soon as certain circumstances arose, the material might be fractured in an alarming way. No doubt many members had heard of the extraordinary accident that happened to a set of marine boilers, an account of which had appeared in "The Engineer."[1] In that case he had been much struck with the want of consistency between the tests made of the material after the accident, and those that had been made before. One thing had especially attracted his attention, that whereas the average tensile-strength of the material by the tests at the works was from 27 to 28 tons per square inch, in no case exceeding 30 or 30·3 tons, and in no case sinking below 26·5 tons, when it was tested after the accident it was found that some of the steel had sunk as low as 24 tons; and another plate, which apparently belonged to the same quantity of steel, and presumably had gone through the same tests, had risen to 40 or 47 tons. Assuming that the steel had altered its condition between leaving the works and the time of the accident, it was hardly likely that the same conditions would degrade the tensile-strength per square inch from 27 or 28 tons to 24 tons, and also enhance it to 40 and 47 tons. That discrepancy seemed to suggest that the whole of the steel had not been thoroughly tested, and that consequently testing had been blamed for a defect that was really personal, the test not having been carried far enough. Some years ago, when mild steel was first brought into use for shipbuilding purposes, a case happened that gave him a great deal of concern. Some of the angle-bars for forming deck-beams were being straightened and set in the squeezers previous to being put into place. The rivet-holes for the deck-plating came to about the middle of the flange of the bar. On being set in the squeezers, bar after bar flew out from the hole to the edge.

[1] December 11, 1885.

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That was rather alarming, because it was very short, and indicated that the metal was not trustworthy. Further experiments with some other bars showed that the holes, instead of flying out suddenly and sharply to the edge, elongated, and the material on the tension side of the hole became locally contracted, and was pulled down like a piece of toffee. That suggested to his mind the desirability of instituting a test which he had called the edge-bending test, and which he had frequently adopted with satisfactory results. The method was to take a piece of plate or bar, and punch or drill a hole in it, and then apply the edge-bending test. If it flew suddenly from the hole to the edge of the bar or plate, he considered that the material was hard, or defective in some way, that would justify him in condemning it. If, on the other hand, it elongated, as in the case he had spoken of, he thought the material was trustworthy. He had never known that test to fail. With reference to the testing of steel, his impression was that if it was carried out systematically and thoroughly, in a way in which he believed Lloyd's were now carrying it out, practically examining every plate, the testing would be sufficient to show the condition of the material. The question of blue heat required a much larger number of experiments before engineers could satisfy themselves as to the points raised by the Author. Mr. West.

Mr. W. PARKER said that it had been well known for a long time to the authorities at Lloyd's and the Admiralty that steel at a blue or black heat was brittle; but it had never occurred to him, nor, he believed, to the Admiralty authorities, that the brittleness was brought about by putting work on the steel when at that temperature. That, however, opened up a very grave question, for it had been the practice of numbers of engineers to work steel at those heats, not only in flanging but also in bending mild plates. When mild steel was introduced for boiler-making and ship-building purposes, in 1878, it was thought that it did not require any different treatment from iron. It had been often stated that this material could be knocked and hammered about as much as iron with impunity, but he thought that every one who had had experience in working steel knew that it could not be so treated. It was, as had been stated, a different material from iron, and should be worked in a different manner. One of its peculiarities, as the Author had pointed out, was that it became extremely brittle when worked at these dangerous heats. The late Sir William Siemens had endeavoured on many occasions to show why peculiar fractures took place in steel plates. He Mr. Parker.

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Mr. Parker. represented steel as being of a homogeneous, and iron of a heterogeneous nature, iron having fibre in it, and steel none; and he went on to say if strains were set up by local heating or by shearing or punching, and they were more than sufficient to overcome the strength of the plate at any particular point, a fracture or tear would certainly take place, which many persons might regard as "mysterious." It had been Mr. Parker's privilege since 1878 to investigate nearly all the accidents of importance that had occurred to steel plates in this country, and he would briefly explain a few typical cases. The first of any importance was on the Tyne. Two steel boilers were being constructed for the mercantile marine. One of them was made satisfactorily, but the other met with an accident. The boiler was about 10 feet in diameter, and built to work at a pressure of 70 lbs. to the square inch. It contained three furnaces, which were riveted to the tube-plates, and to the front-plates by a single row of rivets. The boiler was almost completely riveted; in fact the men were just completing the riveting of the front edge of the furnace, when they left the boiler to go to dinner. On their return an hour afterwards, to their great surprise the furnace-plate had torn for a distance of about 18 inches. Everyone then said that the plate must be a bad one; but on analysis it was found that the material was as good as could be made, having come from the best makers in the country at that time, namely, the Landore Steel Company, and been made by the Siemens-Martin process. The tenacity of this plate was not more than 28 tons to the square inch, and it had an elongation of over 26 per cent. in a length of 8 inches. By accident the bolts that had been holding the furnace up to the mouth-piece or front-plate of the boiler had not been taken out. When they were taken out it was discovered that the rivet-holes, although originally drilled through both plates in place were very unfair and quite $\frac{1}{8}$th of an inch blind, proving that the furnace itself had originally been much less than the circular part of the front plate to which it had to be riveted. The men in riveting stretched the furnace-plate and made it accommodate itself to the size of the front plate, and threw strains upon the material, sufficient to tear it when the proper time arrived. Had this not been found out, the plate would have been considered to be bad steel. It had been said that when an accident occurred to a steel plate a great deal of noise was made about it, while nothing of the sort took place with regard to iron. For his own part, he thought that whenever engineers met with any "mystery," it was their bounden duty to try and get to the bottom of it, in

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the interests both of steel-makers and steel-users. He had therefore taken great personal interest in all such cases that had come before him. The next case was that of a set of boilers, which were made for the Emperor of Russia's yacht, "Livadia," by Messrs. John Elder and Co., of Glasgow. This vessel was to be fitted with eight boilers, to work at a pressure of 90 lbs. per square inch. The shells were 14 feet 6 inches in diameter, and $\frac{3}{4}$-inch thick. The steel was obtained from one of the best makers in the kingdom. The boilers were not under the personal inspection of Lloyd's surveyors, but the steel was tested by the makers in the usual manner, and also by Messrs. John Elder and Co., and pronounced to be quite satisfactory. When water-pressure was being applied to the first boiler, before it reached anything like twice the working pressure, the shell of the boiler tore across the plates in three places, and on examining another of the boilers that had not been tested it was found to be cracked in five or six places without having had any pressure upon it at all. The shells were taken off the whole of the eight boilers and new shells made, but even at that time, and in face of the apparently unaccountable accident, so much confidence was felt in steel that iron shells were not substituted, steel being ordered from works on which more reliance could be placed. He had been requested by the Committee of Lloyd's Register to try to get to the bottom of the "mystery." He went to Glasgow and had a piece of the plate sent to London, where he found it in its then condition to be so brittle that it could be broken with an ordinary hammer; but when it was annealed and the strains that existed in it thus set at rest, it was perfectly ductile and bent double without any signs of fracture. The tenacity of the plates was everything that could be desired, and so were the elongation, ductility, and chemical analysis. It was ascertained that after the plates were rolled they were piled one on the top of another in an annealing furnace, and sent from there to the shipyard at Glasgow. It occurred to him that the annealing done in this manner might have set up some strains of a serious nature in the plates, but nothing could be stated definitely on this point. An experiment was made with a view to ascertaining whether the plates had had sufficient work put upon them. A piece of the plate was rolled down to half its original thickness, and when tested it was found that after the extra work had been put into the material, it could be bent, or doubled, or anything done with it. This result seemed to point to the conclusion that the plates had not had sufficient work put upon them, but he was never perfectly satisfied with that

Mr. Parker.

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Mr. Parker. conclusion. He was, however, satisfied that the steel in its original state was, unlike the material in the other case referred to, bad steel, and quite unfit for the purpose for which it was intended. The next case to which he would refer was that of a boiler made in the beginning of last year at the Wallsend Slipway Company's works on the Tyne. It was about 14 feet in diameter, and constructed to work at a pressure of 150 lbs. per square inch. The plates that gave way were shell-plates, and stood all the mechanical tests that were required. They were 20 feet long, 5 feet 6 inches in width, and 1¼ inch thick. When the boiler was being tested the shell burst. The pressure was being raised to 300 lbs. per square inch, and as it reached 240 lbs. the plate tore through from end to end. In endeavouring to get to the bottom of this occurrence he had pieces cut from the plate and tested, and they quite corresponded with the tests which Lloyd's surveyors had carried out at the steel works on specimens of the material; the analysis of the plate was found all right, except that the amount of carbon was twice as high as in any other plate he had ever seen to stand the same mechanical test. This was so striking that he looked further into the matter. He visited the steel works, and the makers assured him that a plate of 30 tons to the square inch, and 1¼ inch in thickness, must necessarily be much higher in carbon, because the mechanical work put upon it was not nearly so much as in the case of a plate ½ inch thick. They told him that every thick plate thus required more carbon in it than a thin plate intended to stand the same mechanical tests. The makers took him round their works, and, pointing out different ingots, said, "These ingots are for ½-inch plates intended to have a tensile-strain of 30 tons to the square inch; those are intended for plates an inch thick, intended to stand a strain of 30 tons; and those are intended for plates 1¼ inch thick to stand 30 tons; and the carbon, which is the tempering property, varies in every one of them." He read a Paper before the Institution of Naval Architects on this subject, in which he endeavoured to show that with the enormous plates now used in high-pressure boilers, engineers were, perhaps, unknowingly drifting into a somewhat dangerous material;[1] and from that day to the present, Lloyd's Register Society had endeavoured to discourage rather than encourage large thick plates of high tenacity. The next case, which differed from all the preceding ones as far as the investigation went, took place only a fortnight ago. It was that of a boiler which was being made

[1] Transactions of the Institution of Naval Architects, vol. xxvi. p. 253.

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at Hull, about 10 feet in diameter, to work at only 90 lbs. pressure per square inch. It was riveted and complete, and while being tested, the shell-plate failed, tearing through from end to end, much in the same way as in the last case, one part bulging considerably, and the other remaining in its normal position. This plate was only $\frac{11}{16}$ inch thick, and had never been heated at all. So far as he could ascertain, the analysis was everything that could be desired, as were also the tensile- and bending-tests; still the plate cracked when only one-third of its tested strength had been reached. That brought to his mind a remark made by Mr. Gilchrist, that it was not uncommon to roll plates when one end was colder than another, or to roll them while cold, and deliver them to the users with initial strains existing in them almost sufficient to tear the plates without their being worked. In such a case the test-piece might give a satisfactory result and yet convey no idea of the real state of the plate itself. This of course could be avoided if every plate were properly annealed after having been rolled and sheared. Mr. West had referred to the boilers in the "British Prince" and "British Princess," which had caused so much commotion. The material in that case, and the boilers themselves, were made four or five years ago, so that it was ancient history so far as Mr. Parker was concerned. The material was tested by Lloyd's surveyors, and it was well known that in this particular case they had to condemn a great deal more material than they passed. Their tests were not at that time of such a nature that they could test every plate. Figs. 8, Nos. 5 and 6, represented the method adopted of bending a plate for testing purposes. The works where the plates were made had small Bessemer converters, the first, he thought, that Sir Henry Bessemer had made, and were only capable of turning out a charge of some 30 cwt. each. The plant, rolling-mills, &c., were of the lightest description; the hammers were not heavy enough to put a sufficient amount of work on the material, and very thin and very small plates were habitually made. In this state these works branched out into the marine-boiler trade, and it could not be wondered at if the manufacturers got into trouble at first. It was their practice to cast the ingots flat, in the form of a slab, about 5 inches thick, instead of casting them in the form of an ingot of about 20 by 20 inches thick; these slabs they heated and passed through the mill, and produced plates from them. The plates in question were made at those works, and though they were tested, they seemed by some means to have escaped the notice of those who were responsible for the quality of the

Mr. Parker.

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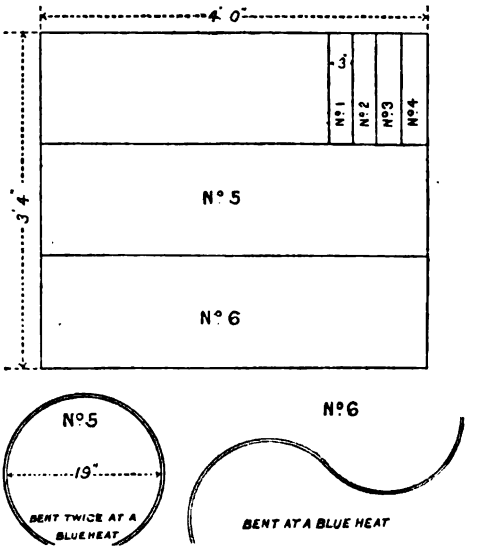
Mr. Parker. material. The makers, seeing that their existing plant was not powerful enough to make a satisfactory material for marine boilers, had since put down Siemens furnaces, had erected large hammers, and now turned out of their mills as good steel as any firm in the kingdom. With reference to the remarks of the President as to the increasing use of steel, he might be permitted to say that

FIGS. 8.



METHOD OF BENDING A PLATE.

when the first mild steel marine boiler was being made in 1878, he had the honour to read a Paper before the Institution of Naval Architects, the concluding remarks of which, if it were not considered egotistical on his part, he should be glad to read:—

"Now as we have a material that gives us a boiler about 30 per cent. stronger than an iron boiler of the same scantlings, and as it seems possible that we may be able in the immediate future to dispense entirely with longitudinal riveted seams by having the shells rolled, and as there has also been a furnace introduced which can be worked at twice the pressure of the ordinary plain flue, it does appear to me that we have succeeded in a great measure in removing the old conditions that have militated against much higher pressures being obtained, and that we appear to be now in a position to make a fresh departure in the direction of still greater pressures. If the improvements which I have indicated prove, as I have little doubt they will prove, successful, we shall have gained an advantage represented in the aggregate by an increase of about 80 or 90 per cent. of the working pressure. In other words, we shall be able to work the present form of boiler at 160 or 170 lbs. per square inch; and, although the resultant economy will not be so great as that which attended the increase at one step from 30 to

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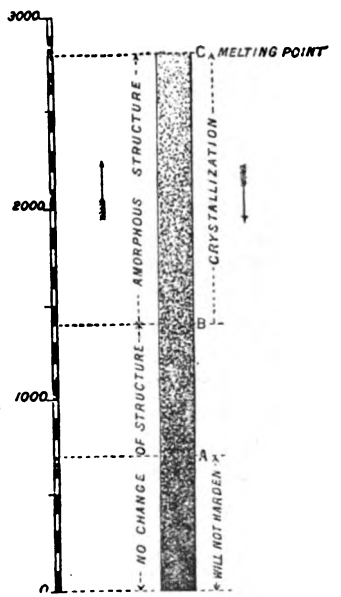
60 lbs., we may confidently anticipate that it will be sufficient to give a great impetus to steam navigation, advancement in which has lately been so much retarded by the high consumption of fuel."[1] Mr. Parker.

He was glad to say that, with the exception of rolling shell-plates solid, all this had been attained; and up to the present time there had been made over 4,000 marine boilers, representing over 160,000 tons of steel, and there had not been one accident under steam. Besides that, the advancing pressures had brought to the front an engine—the triple expansive engine—which had conferred upon the shipowners of this country a boon, represented by quite 20 per cent. less coal than was burned before the introduction of steel. He believed he might safely say that the introduction of this material had been the means of an improvement in marine engineering such as had not been witnessed during the last twenty years.

Mr. W. ANDERSON said that the Author of the communication under discussion did not appear to be aware that in 1868 Mr. Chernoff read a Paper on the effect of temperature on steel, at a meeting of the Imperial Technical Society of St. Petersburg. Mr. Anderson translated that Paper in 1876 under the auspices of the Institution, and the Steel Committee of the Institution of Mechanical Engineers republished the translation.[2] Mr. Chernoff, as forge-master of the Abouchoff Steel Works, had unrivalled opportunities of studying the behaviour of steel in forgings of from 40 tons weight downwards. The theory which he propounded was this: he divided the whole range of temperature from 32° to the melting-point of steel into three zones (Fig. 9). Steel confined to the temperatures in the lowest zone, the limit of which he called A degrees, could not be hardened, no matter how energetically cooled; steel raised to any temperature in the middle zone, the superior Mr. Anderson.

FIG. 9.



CHERNOFF'S THEORY, 1868.

[1] Transactions of the Institution of Naval Architects, vol. xix., p. 181.

[2] Proceedings, 1880, p. 225.

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Mr. Anderson. limit of which was at B degrees, did not undergo any molecular change, though some qualities would harden on being suddenly cooled; and, finally, steel heated above the temperature B adopted an amorphous or wax-like structure, and became very plastic, the said plasticity extending up to the melting-point. He next drew an analogy between the behaviour of a hot concentrated solution of alum, and steel in the highest zone. It was well known that a hot solution of alum, if allowed to cool slowly, would solidify in large crystals; if allowed to cool slowly, but if kept agitated, it would solidify into small crystals; if made to cool rapidly it would also form a finely crystallized solid; and, finally, if made to cool rapidly, and if kept agitated as well, the finest crystals would be formed; and so it was with steel. If heated above the point B and allowed to cool slowly the metal would become coarsely crystalline and unfit for use. If cooled slowly, but well worked all the time, a fine silky grain would be obtained, and this result would be still more apparent if the cooling took place moderately quickly. If cooled suddenly, as in tempering the large masses of metal forming guns, a fine and uniform grain would result. The temperatures A and B varied with the chemical composition of the steel. In pure steel, a combination of iron and carbon only, Mr. Chernoff stated that the temperatures became lower in proportion as the percentage of carbon increased. He had prepared some samples of boiler-plate for the purpose of illustrating Mr. Chernoff's views. He had four specimens cut from the same lot of plates, and annealed them all by heating them above the temperature B and then allowing them to cool quickly on an earthen floor. The samples were, by this means, all brought to the same molecular condition. No. 1 was doubled up flat under a steam-hammer cold, and then broken open. Not the slightest crack was observable on the outside of the bend. No. 2 was heated to a blue heat and doubled up under a steam-hammer, then broken open, and no crack whatever could be detected. No. 4 was kept at a bright-red heat in a hollow fire for ten minutes, then lightly but rapidly forged by two hammers on a cold anvil till the temperature had fallen to a blue heat; it was then doubled up under the hammer and broken by opening out. Not the slightest crack could be detected. Comparing the fractures of the three samples, no difference could be detected. No. 3 was heated to a bright-red heat in a plate-furnace last Saturday, and was allowed to cool very slowly with the furnace. The specimen was then doubled up cold and broken open like the others. No distinct crack appeared on the outer surface of the bend, but

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the strain had revealed the boundaries of a crystallized structure by a network of incipient cracks, and on the inside two cracks had started besides the one at which the bar broke. The fracture was much more coarsely crystalline than that of the other specimens. This showed that the very perfect annealing received was a positive injury, a circumstance which Mr. Chernoff's theory would suggest. Mr. Kirkaldy had noticed this effect, for he stated that iron was injured by being brought to a white or welding heat if not, at the same time, hammered or rolled. Now it was evident that had the Author got hold of sample No. 3 he would have found it brittle at a blue heat, whereas if he had heated it first above B and then cooled it quickly, a contrary result would have been obtained; consequently to have made his observations valuable, he should first have annealed all his specimens in the proper manner so as to bring them all to the same molecular condition. In rolling boiler-plates it was manifest that, if the plates left the rolls at temperatures higher than B, and were allowed to cool slowly in heaps, those parts of the plates which were slowest in cooling would crystallize in coarse crystals, while the parts which cooled more rapidly would assume a finer grain, and internal stresses would consequently be set up, which would tend to make the plates crack spontaneously sooner or later, and this effect would be more pronounced with thick plates than with thin ones. He was convinced that it was from ignorance of this point that the apparent quality of steel plates was so uncertain. Iron was not so sensitive because the temperature B was very high, and iron plates always left the rolls well below it before being allowed to cool. He was at present engaged in condensing, for the Minutes of Proceedings, an account of some experiments on steel and iron girders translated from the Proceedings of the Dutch Institution of Civil Engineers, by Mr. Siccama, M. Inst. C.E., and he found there a record of two steel beams the plates and angles of which were annealed before the beams were put together. To the surprise of the experimenters the beams behaved worse than other girders of soft steel, hard steel, or iron. It was not stated how the annealing was done, but he presumed in the same way as a whole beam was annealed, namely, by putting the plates into a furnace, heating them up to a high temperature, and then allowing them to cool slowly with the furnace. If the temperature attained was higher than B, Mr. Chernoff's theory explained the cause of failure, and the fact that samples cut from the plates showed high tensional resistance would be quite compatible with the observed results. He believed that it would be in vain to look for greater

Mr. Anderson.

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Mr. Anderson. certainty in the behaviour of steel until more attention was paid to the points raised so intelligently by Mr. Chernoff, although, undoubtedly, many other considerations depending upon changes in chemical composition or structure, due to alterations of temperature, which had been so carefully investigated by Sir F. Abel, should not be lost sight of.[1]

Professor Unwin. Professor W. C. UNWIN said that in spite of Mr. Gilchrist's protest he was afraid that steel behaved sometimes in a mysterious way, in a way which could not be adequately explained, and he thought it was of the first importance to get to the bottom of the explanation of the so-called failures, because until then it could not be known whether the empirical tests by which its quality was judged were the best tests that could be used. There was, he thought, sometimes a tendency to accept explanations as adequate that did not completely solve the difficulty. He might be permitted to refer to a case mentioned by Mr. Parker to show what he meant. It was a case of a boiler-flue which cracked, the steel being, as Mr. Parker believed, perfectly good. The flue was being riveted up to the end plate of the boiler; the circle of the rivets was partly completed; the men went to dinner, and while they were away the plate cracked. It was afterwards found, when the plate was taken out, that the rivet-holes had become foul about $\frac{1}{8}$ inch, and Mr. Parker put that forward as an explanation of the accident. On that two questions arose; first, why the plate did not crack when the stress came upon it, why it waited till the men had gone away? and, secondly, which was a more important question, how it was that in a material which would elongate 30 per cent. before fracture, the amount of strain indicated by the holes becoming foul to the extent of $\frac{1}{8}$ inch produced a fracture? The flue was 9 feet in circumference, and with 30 per cent. of elongation ought to have lengthened 3 feet before cracking. Again, looking at the enormous strength of the boiler-plate, it was difficult to see how the workmen could put strain enough on it to crack it if it were perfectly homogeneous. With reference to the Paper, he would pass over the experiments on tension, because the Author had drawn no definite conclusions from them. He would simply observe, with respect to the diagrams of strain and stress, that some experiments by Mr. Martens, in the laboratory at Berlin,[2] seemed to show that when the elongation was obtained in that way, the curves were liable to errors + or − 5 per

[1] Institution of Mechanical Engineers. Proceedings, 1881, p. 696; 1883, p. 56.

[2] Ueber die Bestimmung der Zahigkeit der Materialien. Mitt. aus den K. Tech. Versuchsanstalten zu Berlin, 1884, p. 93.

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cent.; that would almost explain the irregularity of the curves. There were two series of results in the Tables which were very important in considering fractures of steel. First, there were cases of plates which were bent while at a blue heat or a black heat, and which broke with a very small amount of bending. It had been known for a long time that there was a temperature below the temperature at which steel was plastic, at which it was rather dangerous to work it. The Board of Trade, in 1881, had published memoranda giving a series of experiments carried out at the Steel Company of Scotland's works. Forty-eight plates were taken, and a strip was cut from each. Half of the strips were bent cold to an angle of 180° round a bar of a diameter equal to twice their thickness. The whole of the plates bent cold stood the test. Corresponding strips were bent at the temperature of boiling tallow, and every one of the strips bent hot cracked long before 180° was reached. But he did not think that these results threw much light on the mysterious behaviour of steel, because engineers had no right to assume that either the physical or the chemical condition of the steel was the same when hot as when it was cold. The most striking results no doubt were those which showed that a plate, which had been bent hot—at a blue heat—and then allowed to cool, was very brittle after it was cold. Those results were no doubt much newer. The Author explained them by saying that the plates had lost their ductility, and he supposed that must have been so. But there arose the question, why was it not possible to detect loss of ductility in the tension tests? The results were of course extremely interesting, and he had no doubt that qualitatively they indicated exactly the conclusion that the Author drew, that the steel was dangerous after it had been treated in that way. But the failures in steel which were most interesting did not occur while the plates were being bent, but while the steel was being subjected to the ordinary simple conditions of stress, and there was therefore a gap in those experiments in regard to the explanation of the failures of steel. He had lately tried to ascertain whether the tension-tests would not afford information as to the condition of steel after it had been treated in that way. He had taken a plate of 27-ton steel and broken it in the ordinary way. The longer half he formed into a new test-piece, and he treated the part to be tested to a temperature a little below redness, and quietly bent it in the testing-machine, and also straightened it in the testing-machine. It was not bent to any sharp angle, but only to a comparatively shallow curve without hammering or vibration.

Professor Unwin.

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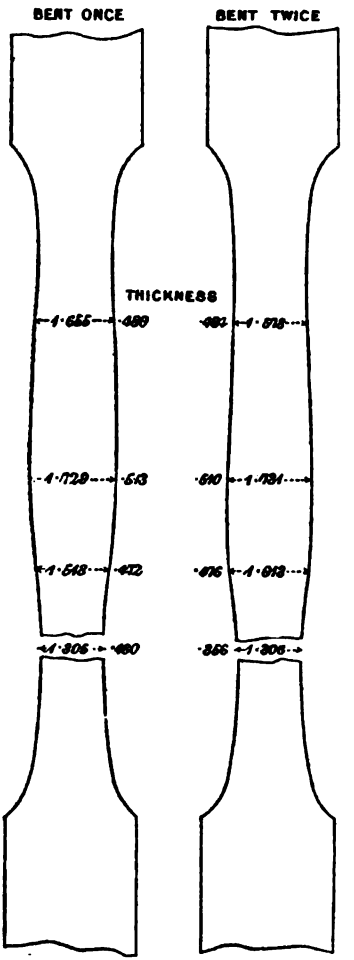
Professor Unwin.

The plate was then tested by tension. In the first testing it stood 27 tons to an inch, in the second it stood 32 tons; in the first testing the contraction of area was 51 per cent., and in the second it had gone down to 21 per cent. He then proceeded in another way. He took some test-pieces of the same mild steel, which were heated in a forge to a good red heat. He then placed them in the testing-machine between an ordinary V block, and a large round piece of iron, and quietly bent them to an angle of about 15° on each side from their original straight position. The pressure was put on when the redness disappeared. They were straightened in the testing-machine in the same way, and then tested as ordinary test-pieces. Figs. 10 represented the result obtained. In the centre of the curvature, where the bar

FIGS. 10.



ELONGATIONS OF 1 INCH.

0·048			
0·152		0·120	
0·138		0·154	
0·080	Heated.	0·156	
0·020		0·064	
0·010		0·000	Heated.
0·138		0·002	
0·287		0·082	
0·622	(Break)	0·174	
0·160		0·610	(Break)
0·100		0·232	

was bent, there was hardly any measurable contraction of area, and hardly any measurable extension. On each side of the middle of the bar it was drawn away in the form shown, but somewhat exaggerated. The figures showed the widths and thicknesses of the bar. It would be seen that the bar had contracted everywhere except just in the middle. Over the heated part the elongations, it would be seen, were very small, and towards each end they were much

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larger. This proved that that treatment of the steel rendered it unhomogeneous, and it could be easily understood how by treating a plate of steel in that way it might be rendered very untrustworthy. Suppose the rigid part of the metal, for example, to be near the edge of a plate, and the strain in the direction of its length, then the rigid part of the plate would have to carry nearly the whole load on the plate, and an unhomogeneous plate of that kind would no doubt crack, under a load which apparently did not exceed the working tension of the steel. He was not putting that forward as anything like a complete explanation of all that occurred, but he thought it showed one way, at all events, in which the unhomogeneity of the plate might arise, and in which a dangerous condition of the plate might possibly exist. He had tried another experiment with steel, which hitherto had not been successful—making cuts with a diamond to ascertain if he could reduce the plate's strength. At present the plates generally were not affected by the cuts, and the strength of the bar was not much altered. It had been often stated that failures in steel were due to initial stresses in the metal. Mention had been made of the way in which castings of steel or of pieces of glass would crack under apparently no load, in consequence of the very large stresses existing in the material itself, owing to the way in which it had been cast. It had always seemed to him to be exceedingly difficult to imagine that in a tough material like steel there should be initial stresses capable of tearing a bar to pieces. A material which could extend like steel, would, he imagined, relieve itself of any conceivable initial internal stresses without cracking; but if the steel itself had different extensibilities in different parts, then the excessive stress in certain parts was due to the loading. It was not the initial stress in the material, but the want of uniform distribution which led to failure. Professor Unwin.

Mr. THOMAS W. TRAILL said that according to his own experience the working of steel at a blue heat was a very imprudent thing, but there was something still more dangerous, and that was, after putting work on a hot plate, to place it in the structure before it was efficiently annealed. He believed that nine hundred and ninety-nine failures out of one thousand had taken place with steel of the mildest description, and having a good elongation and a low tensile strength. There had been comparatively few failures with steel for shell-plates, but they had taken place in nearly every instance with plates that had had work put upon them. In the case of the "Livadia," mechanical tests had shown that the steel Mr. Traill.

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Mr. Traill. shell-plates had a mean tensile-stress of about 26 tons, and an elongation in 10 inches of about 27 per cent. Unfortunately those boilers failed under the hydraulic test at a strain of about 8 tons per square inch net section. A joint cut out of the boiler gave way at a stress of about 9 tons per square inch net section. Two joints, constructed out of the plate in a similar manner to that in which they had been originally constructed in the boiler, gave way at about 11 tons per square inch net section. But there were other joints constructed after the plates had been annealed, and they did not break until the stress reached about double that of the joints that had been made with unannealed plates, but these plates were probably affected by the punch more than some others. There were still some engineers who were afraid of using steel; but he could safely say that, considering the number of iron marine boilers that had been constructed since mild steel had come to be used during the last ten years, the percentage of failures in iron boilers had been considerably above those in steel boilers—a fact that ought to be consoling to those who were in doubt with reference to the adoption of the useful and trustworthy material, steel. Steel, like other metals, required peculiar treatment; and if manufacturers and boilermakers would not give steel the kindly treatment that its peculiarities demanded, they must expect to have a few failures. Professor Unwin had alluded to some experiments that had been published in 1881. Mr. Traill conducted those experiments himself seven years ago, and he could corroborate what Professor Unwin had said about the failure of the specimens heated to the temperature of boiling tallow. The reason why he had adopted that mode of heating was to ensure that all the specimens should be of a uniform temperature, and in bending them at that temperature they failed. In writing the report conjointly with his assistants, he had remarked that no branch of experimental research was more worthy of attention than the properties of mild steel at various temperatures up to 1,000°; but he was sorry to say that no one had yet come forward who would spend the money required, for it was a costly thing to conduct a series of trustworthy experiments to prove exactly at what temperature steel became dangerous. He had almost hoped that he had persuaded a steel-maker to have a series of such experiments made; but the cost had frightened him, and until some energetic steel-maker came forward for the purpose, engineers would still be doubtful as to the employment of steel in structures, when its temperature was liable to become moderately high. If, however,

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they would only watch the workmen, they need not be afraid, because there had been no case of steel failing that could not be traced to improper treatment after it had left the hands of the manufacturers. The failures had not been due to the manufacturer, except perhaps in some solitary case of sending out a bad plate. It certainly was not to the interest of the manufacturer to do so, and the tests prevented it. As to chemical tests he was afraid that they would be too troublesome and expensive for practical engineers to adopt. If mechanical tests gave good results, showing a fair amount of ductility, not less than 20 per cent. in a length of 10 inches, although he should like to see about 5 per cent. more, he thought there need be no fear of using that material. One failure, however, had taken place that had not yet been investigated, and he did not know any one who was able to state exactly the cause of it; but possibly that steel had not been made by a process like that employed for steel hitherto used in the construction of marine-boilers. In using steel for the shells of boilers, engineers knew very well that the plate was subject to little or no work, and those who adopted the practice of heating the plates before they bent them must expect to have failures. But there were engineers who rolled the plates cold up to 1¼ inch thick, and they said, "If the steel-maker cannot supply us with a plate that will stand rolling without injury, we will not have his plates." He advised all who constructed boilers to prohibit heating when rolling plates for the shells. Mr. Traill.

Mr. W. H. Barlow, Past-President, asked, if Mr. Traill referred to the process of annealing, to what temperature the material was raised, and in what manner it was cooled? Mr. Barlow.

Mr. T. W. Traill said he was not speaking of experiments in annealing, except in the case of the joints he had referred to. The material was heated to a bright red, and allowed to cool gradually. Mr. Traill.

Sir Frederick Abel said he was not quite sure that chemists had made themselves sufficiently heard to convince the members of the Institution that the chemical investigation of the properties of steel was of the importance that they, the chemists, attributed to it. The Author had stated that his earlier experiments were made at a time when the peculiar influence of a blue heat was less understood than it was now. That had led Sir Frederick Abel to look through the Paper carefully to see how the influence of the blue heat upon the steel was explained, and he confessed that he was there at fault. It had also led him Sir F. Abel.

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Sir F. Abel. to consider whether any knowledge that he might have acquired, in the laborious examination in which he had taken some part in connection with an inquiry into the condition in which carbon existed in steel, might contribute to an explanation of the peculiar behaviour of steel pointed out in the Paper. The interesting observations of Chernoff, to which Mr. Anderson had directed attention, he believed went far to explain such results as had been observed by the Author. At the same time, he thought that certain results which he, together with his assistant, Mr. Deering, had arrived at, in their examination of samples of steel, might possibly have some bearing upon the question, and he would therefore throw out a few suggestions on the subject for the consideration of the members. In the course of the investigation it was found, in the first instance, that in annealed steel the carbon existed almost entirely in the form of an iron carbide of very definite composition, uniformly distributed in an extremely fine condition throughout the mass of the metal; and that, on the other hand, in hardened steel, it appeared as if the passing of the steel into the hardened condition did not allow time for the separation or elimination of the carbon in that particular form; that, in fact, the carbon existed in steel in the hardened form almost entirely as it existed in solution in the liquid metal. Very small proportions of the definite carbide were found in the hardened metal, but they were probably the result of some small amount of annealing which even hardened metal underwent in the course of cooling. The results which they found in examining steel which had been submitted to degrees of heat included under the general term "blue heat," were intermediate between those exhibited by the annealed metal and by the hardened metal. He had with him two specimens, which might serve to illustrate what he meant as to the condition of the carbon existing in the steel. In the vessel which he exhibited there was a small platinum sieve (glass sieves were also used with fine perforations), upon which the little steel disk operated upon rested, in a liquid by which the iron was very slowly dissolved. As the iron dissolved the carbide of iron passed through the meshes of the sieve or the minute perforations in the glass, and collected at the bottom as a powder of a fine and uniform state of division. In the specimen exhibited, it was fixed by means of gelatine, and it showed that the carbon was disseminated throughout the solution in an almost impalpable state of division. He had also a sample showing the result of a corresponding treatment of a portion of the same steel, blue-tempered, and in that case instead of the carbon passing through

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Sir F. Abel.

the sieve in a state of almost impalpable powder, or in a very finely granulated form, the portion which existed in the specimen as carbide of iron remained upon the sieve in a comparatively coarse condition, and had not been uniformly distributed throughout the original mass. It presented, therefore, in regard to the condition in which it existed in the metal, a decided difference as compared with the carbide from the other annealed sample. It was evident that the difference in the condition of the carbon existing in annealed steel, and in the tempered steel might exert an important influence upon the strength or the behaviour of the metal, for if the carbide was not uniformly disseminated throughout the steel, but irregularly distributed in comparatively coarse portions, it might seriously affect the working properties of a specimen which had been subjected to a particular heat, favourable to the partial separation of the carbide. It had occurred to him, on looking at the results obtained in the course of their investigations, that these circumstances might possibly contribute towards explaining the causes of the behaviour of steel when submitted to the action of blue heat. For example, whereas in the case of annealed steel they had separated from particular descriptions of steel from 12 to 13 per cent. of carbide of iron, containing almost the whole of the carbon in the steel, while in hardened steel there was always much less than 1 per cent. thus separated, in the case of steel of a straw temper there were separated 7½, 4½, and 5 per cent. of the carbide, and in the case of blue-tempered steel between 4 and 5 per cent., so that the proportions were intermediate between those found in the hardened and in the annealed metal. He did not ask the members to lay very great stress upon those facts, but they might possibly indicate one of the conditions affecting the behaviour of the metal when subjected to blue heat. He had felt some regret in noticing that it was rather difficult to compare the results produced in the different experiments by the Author, because the difficulty (referred to by a previous speaker as a formidable one) of submitting the samples to chemical examination had not been faced. No true history had been given of the specimens of steel; they were only distinguished as coming from different makers, and as being mild or harder steel, so that it was very difficult for a chemist to draw any inference from his point of view as to what might be the causes of the differences observed. The Author had spoken of the effect of baking in a core-moulder's oven some samples which he had exposed to the different tests. It was somewhat difficult to reconcile the different

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Sir F. Abel. results, and to offer anything like a satisfactory explanation. Yet even there he found that the results obtained in his examination of samples of one and the same specimens of steels submitted to different treatments, showed that the condition in which the carbon existed in the metal, whether in large or small proportions in the form of carbide, or in the more direct union with the iron throughout, seemed to have an influence upon its properties, and to be to some extent a means of explaining the results obtained. Among the specimens with which he had been furnished by Mr. Paget in carrying out his investigations, were some samples of blue steel, which had been heated only for fifteen minutes, while others had been heated for six hours, and he had found that the proportions of carbide of iron existing in the specimens bore a direct relation to the period during which they had been exposed to heat. In the case of two specimens of the same metal, one of which had been heated fifteen minutes to a blue heat, and the other tempered for six hours, the first yielded 2·37 per cent. of the carbide, and the second 4·61: there was, therefore, an increased proportion of the carbide formed, and possibly an increased effect in the direction of want of homogeneousness of the metal consequent upon a longer exposure to a blue heat. In another set of experiments, the blue steel, which had been treated for fifteen minutes, furnished 3 per cent. of the carbide, while a portion of the same steel which had been heated for six hours furnished 6·6 per cent. The specimens were disks that had all been cut from the same piece and treated exactly in the same way. In a third set of experiments he obtained corresponding results. The specimen which had been heated fifteen minutes furnished 3 per cent., while that which had been heated for six hours, furnished 6·6 and 6·76 per cent. The facts that he had mentioned might perhaps add somewhat to the information which the discussion was intended to elicit.

Mr. Baker. Mr. B. BAKER had observed a characteristic feature in the discussion which he regarded as a hopeful one. Every speaker had taken the trouble to make experiments, and if that process was continued he believed engineers would soon come to the bottom of the so-called mysterious fractures of steel. It was no use to theorize without plenty of data. The Author had set an excellent example, his Paper consisting almost entirely of a record of experiments, leaving others to contribute additional facts bearing on the subject. The result he hoped would be that there would be shortly an accordance as to the causes of the fractures. His own experiments had been rather considerable, for at the Forth bridge

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about 1,000 tons of steel girders were now being turned out per month. A considerable proportion of the steel had to be heated, and it was liable to such contingencies as the Author had properly stated it was absolutely essential to guard against. He agreed with the Author in his conclusions as to the result of bending plates cold after they had been subjected to a blue heat; but it had occurred to him that in practice no plate would ever be liable to be bent to an angle of 40° or 50°, if it had once got into the structure. What it was perhaps liable to, especially in the case of boilers, was the frequent repetition of stresses beyond the elastic limit. He had therefore made some experiments in that direction with flat bars treated in different ways. He first took a flat bar and annealed it, and tried how many times he could bend it backwards and forwards, putting the extreme fibres into alternate tension and compression, amounting to about 20 tons per square inch. He found that a bar which had been annealed stood from twelve thousand to eighteen thousand repetitions of a stress of that intensity. He next took a bar which had been bent when hot to an angle of 85°, and he straightened the bar at a blue heat, and put it so straightened into the machine, and he found to his surprise that so far from materially falling off in the number of repetitions which it endured it stood over sixteen thousand. He then took another bar and put it into the machine red-hot, and it was bent through all ranges of temperature from a red-heat till it was quite cold. That bar stood about twenty-eight thousand repetitions, and another bar which had been bent four times at a blue heat stood thirty-two thousand. Every one of the bendings bent the bars beyond the elastic limit to the very severe stress of 20 tons per square inch. That could not, he thought, occur in a bridge, but it might conceivably occur in a boiler if the door were opened and closed, and a current of cold air struck the combustion chamber. But the fact was indisputable that the bending at the blue heat did not reduce the amount of fatigue which the steel would undergo. He did not infer from those experiments that it was unnecessary to guard against injury in the way indicated by the Author. He only wished to point out that it should not be supposed that failure was imminent if by chance a bar which had been bent at a blue heat and had not been annealed got into a structure. Whatever precautions might be taken exceptional cases would occur; but his experiments had shown that the danger was not so great as the Author's experiments appeared to indicate. The Author had dealt with one of the causes of mysterious fractures and one alone; he had not

Mr. Baker.

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Mr. Baker. touched upon the cause which the late Sir William Siemens almost always brought forward, viz., a small crack which started a tear. Sir William often likened the behaviour of steel to that of a piece of india-rubber; but if he had tried the experiment he would have hardly drawn that comparison. It was extremely dangerous to indulge in analogies without making the proper tests. Mr. Baker had compared steel with india-rubber experimentally, and he found that they behaved in entirely different ways, and for obvious reasons. India-rubber was a material which would stretch within the elastic limit 400 per cent.; mild steel was a material which would stretch within the elastic limit only $\frac{1}{2}$th of 1 per cent. On the other hand, india-rubber had a set of only about $\frac{1}{8}$th, whilst steel had a set of $\frac{1}{4}$th or $\frac{1}{3}$th. The curves of elongation were entirely different. The curve of steel was a very flat incline, and then a sharp curve upwards, but the curve of india-rubber was a reverse curve. If an examination was made of the characteristic curves of steel and rubber in relation to the explanation of Sir William Siemens as to mysterious fractures, it would be seen that it was an all-essential element in regard to anything of the character of a tear. So far, as to the known characteristics of the materials as illustrated by the curves of elongation. But going to absolute experiment, he had found that by making a slight tear in india-rubber, the strength of the remaining part of the section was reduced by 60 or 70 per cent. with a fair pull. In the case of one specimen of india-rubber, a fine cut was made on each edge with a sharp knife; it was then loaded, and the strength of the remaining uncut section was only from 30 to 40 per cent. of the original strength per square inch, showing a loss of from 60 to 70 per cent. He did the same thing with steel, making first a fine saw-cut on each side of the specimen, and then raising the specimen nearly to a welding heat—to such a heat that he could close up the saw-cut and render it invisible, so that it was to all intents and purposes a hidden crack. He then put it into the testing-machine, and instead of falling to 30 or 40 per cent. of the original strength of the material like india-rubber, the result was 31·4 tons per square inch (Figs. 11, *b*), (p. 165) the strength of the original bar (*a*), being 32·5 tons, showing a loss of only 1 ton per square inch from the presence of the cracks. Not only, however, was the strength maintained, but the character of the fracture was different in the two materials. In Figs. 11, *g*, a star-shaped fracture would be seen. It might be thought that this specimen was a piece of brittle steel, but it was india-rubber. The fracture of a steel specimen was very different;

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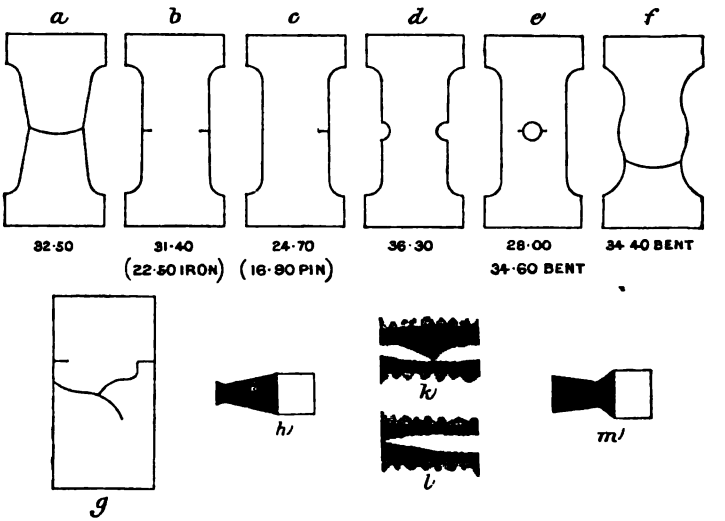
there was a measurable elongation, which could be calculated, and a reduction of section. He had also tried bars, prepared in the same way, but with a fine saw-cut on one side only closed under a temperature less than welding heat. The resistance was 24·7 tons per square inch (*c*), when the bars were held in the usual way in jaws in the testing-machine, and 16·9 tons when held by a pin. The form of the fracture in the first instance was as represented in *h* and *l*; there was an elongation and a gradual reduction of section of increasing amount from the bottom of the crack towards the opposite edge of the bar. Analogous fractures occurred (*k*) with the bars cracked on both

Mr. Baker.

FIGS. 11.



edges. In some discussions at the Institute of Naval Architects it was maintained by several speakers, including Sir William Siemens himself, that in the case of a tear there could not be a reduction of section, but in the instances he had referred to it was so. Of course, when a fracture took place in a steel bar, it usually occurred too quickly to observe the process. In order to follow it the best way was to make specimens in lead. He had adopted that plan, and was thus enabled to see how the reduction of section happened. The elastic limits and the ultimate strength held about the same ratio to each other in lead as in steel. Soon after the elastic limit was passed the crack opened, and became rather parabolic in form, the metal thinning rapidly at the bottom

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Mr. Baker. of the crack (Figs. 11, *m*). As the tear proceeded a wave of thinness passed along the bar from the crack to the opposite edge, the minimum thickness always being at the bottom of the tear, and growing less as rupture extended. It had long been known that if a bar was nicked with circular nicks the strength of the remaining section was greater than that of the original bar. The tensile strength of the steel in the case to which he was referring was originally 32½ tons per square inch in the plain bar, and it went up to 36·3 tons with the semi-circular nicks (*d*). He had also tried the experiment of putting a ½-inch hole through the specimen and making a slight nick on each side of the hole, forming, as it were, a badly-punched hole, so badly punched that the bar was torn for about $\frac{3}{16}$ inch on each side; and even under those circumstances it was not by any means destroyed. The only result was that the strength of the section left was reduced to 28 tons to the square inch (Figs. 11, *e*). He had taken the same kind of specimen and raised it to a blue heat and bent it six times backwards and forwards; but instead of snapping when tested for tensile strength, it sustained 34½ tons to the square inch—more than the original plain bar, which had not a nick in it and had not been tampered with at all. Plain bars bent several times at a blue heat assumed the barrel-shaped outline already referred to by Prof. Unwin, and stood 34·4 tons (*f*). It had been said that steel and iron were entirely different, that steel was a homogeneous material and iron a bundle of fibres. The Author had shown that steel and iron at a blue heat behaved much in the same way. Mr. Baker had shown by his experiments that all that had been said about bundles of fibres or what some persons preferred to call the molecular column, stopping the crack running through, was not justified by the facts. When he took a bar of wrought-iron, nicked it on both sides, shut it up at a welding heat, or a little below, and put on the stress, it stood 22½ tons per square inch on the remaining section, the strength of the iron being 23½ tons, showing a loss of 1 ton to the square inch. The "bundle of fibres" did not help the iron in the least, and the steel was not hurt by being a very perfect material without any cinder between the layers. Each material lost in strength practically only 1 ton per square inch by initial cracks. He did not find that annealing had much effect in cracks of that sort. Of course the injury which steel received from shearing, whether due to cracks or not, was entirely remedied by annealing, and it appeared to him to be pretty conclusive that the injury received from shearing was not due to the initial cracks in a

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metal, because when he made initial cracks purposely he did not obtain that result. He exhibited a piece of steel which looked like a spring: it was a shaving from a 36-ton per square inch steel plate $1\frac{1}{8}$ inch thick, the shaving itself being $\frac{1}{32}$ inch thick. The cracks were so numerous that the inside of the coiled shaving felt like a file, and notwithstanding that, if he annealed the shaving he could flatten it out. He had made a saw-cut $\frac{1}{8}$ inch wide in the side of a bar, and wedged it with a view to split the bar until the cut had widened to nearly 1 inch. He had partially torn bars before testing for ultimate strength, and found the same results as with saw-cuts. He had also cut a specimen from a cracked plate of 35-ton steel, so as to include the last $\frac{1}{2}$ inch of an almost invisible crack, and tested that under tension, and found the crack did not extend until the stress on the remaining section attained 29 tons per square inch. It was indeed difficult to crack good steel, and a cracked plate generally meant a bad plate. Mr. Baker.

Mr. JEREMIAH HEAD said he was one of those who had never ceased to wonder at the marvellous qualities of what was called mild steel, in comparison with the wrought-iron which it had so largely superseded. But perhaps in view of those valuable qualities it was too much to expect that it would not have its own peculiar drawbacks. No rose, it was said, was without its thorn. He was not sure that it was quite creditable to engineers that they should not have found out long since and estimated at their proper value the very elementary qualities which they were then considering. There appeared to be a general concensus of opinion that to work steel at a blue heat was very dangerous, and his own experience led him to agree with that view. It was only the other day that he was passing by a vertical or stack boiler which was undergoing repairs. A ring had been made out of a steel plate in form like an angle-iron ring, to effect the junction of the protruding flue with the flat plate forming the top of the boiler. The ring had been made when the material was at a full red heat, and with perfect success. It had been riveted to the flat plate at the top of the boiler also faultlessly, and it was now being riveted to the flue which went through. Some of the rivets had been put in, and he happened to notice one of those suspicious-looking marks which indicated that there had been some calking done between the rivet and the edge of the plate. That led him to examine it very closely, and he soon found that there were cracks from the holes in various places, not less than half a dozen. He pointed it out to the foreman who said that when he was by the boiler a short time before there was not a crack in the ring. The fact was that it had not fitted Mr. Jeremiah Head.

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Mr. Jeremiah Head.

the flue coming through quite closely, and the men had put heaters on, and no doubt heated it to a blue heat; then closing it with hammers, with the result he had mentioned. He had seen the same kind of thing before. A similar ring was made without any trouble whatever, but requiring a little adjustment after it was cold, if was put on a flat block and heaters were put on one or two places, and very slight blows given to it. Precisely the same result ensued. It cracked in two or three places and became useless. The remedy of course was, as he had pointed out in several instances, to work it either cold or hot, but not at a blue heat. One thing in the Paper had struck him as contrary to his own experience, namely, the assertion that liability to brittleness at a blue heat was common to iron and steel. He had certainly not found it to be a peculiarity of iron, and he could give many proofs that the contrary was the case. Iron-plate boilers were almost always warmed by heaters at the places where an angle-iron or another plate crossed a joint, and needed a little setting in. When heated and closed in in that way he had almost always seen it make a good piece of workmanship, and had rarely seen any symptoms of cracking, unless attempts had been made to do this cold. Again and again when iron plates had been found perhaps rather brittle in bending, and especially when it had been attempted to bend them cold after punching, and perhaps a plate or two had failed, the rest being bent at a blue heat obtained perhaps by a fire of shavings, they were almost certain to bend right. One of the most difficult things was to get an iron plate, say $\frac{5}{16}$-inch thick, and rolled out so that its length was much greater than its breadth, to bend across the grain into a form like the mast of a ship, without cracking. In Holland it had been attempted to bend such plates into masts cold, and the number of failures was very great. Such plates were what was called "reedy," because most of the rolling had necessarily been in the lengthwise direction, and they were apt to split at the ends. But a very little heating, just taking the chill off with a fire of shavings, was generally sufficient to make them bend perfectly well. An engineer, who served his apprenticeship thirty or forty years ago, had probably as one of his first pieces of practical work to put studs into a cylinder. Being at that time made of iron, and when they were put in not always being quite perpendicular, it was customary to give them a blow with a hammer in order to bring them to a perfectly vertical position. Every apprentice knew that if he tried to do that cold he was certain to break two or three of them, and then he had the disagreeable

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task of drilling out the stumps left in the hole; but if he obtained a big nut, or something of that kind, made it red-hot, and put it on the stud and worked it up to a blue heat, he could always set it with the greatest certainty and accuracy. Without, therefore, denying the Author's experiments, he could only say that his own experience had been of a different character, tending to show that the particular drawbacks in iron were not those of steel. Steel was decidedly superior to iron, in the physical qualities of tensile-strength both ways of the grain, contraction of area, and elongation. In those qualities it would beat iron altogether. But in regard to the particular weak point of steel, its liability to brittleness at certain temperatures and to tear, and its liability to behave treacherously at times, it was certainly inferior to iron. Iron, as far as he knew, always showed its worst side first. There was at times trouble to work it up, and it occasionally exhibited defects which were very troublesome, leading to rejections during construction. But if it once entered into the construction it seldom deceived afterwards; whereas steel seemed to disarm all suspicion by the marvellous way in which it at first behaved, but then it seemed liable to behave in a treacherous manner afterwards. Mr. Jeremiah Head.

Mr. W. H. Barlow, Past-President, asked if Mr. Head had made any experiments in testing steel with the grain and across the grain. Mr. Barlow.

Mr. Jeremiah Head replied that he had made such experiments, and that the results were in either direction practically the same. Mr. Jeremiah Head.

Mr. Samson Fox said the time might be remembered when no one grumbled at paying £32 10*s.* per ton for the material with which to make certain boilers, but now there was a great deal of grumbling if £8 a ton were asked for it, and people expected the £8 per ton material to serve with as small a percentage of difficulties as attended the other. Unless engineers were prepared to begin with a high-class raw material, the very best that could be obtained, and to use it by the new process, they would not escape with the low percentage of failures as they might otherwise. He had dealt with between three and four thousand plates during the past year in smithing operations; they had all been intricately formed, and in no single instance had he experienced any of the trouble that had been referred to in the discussion. He had found, like Mr. Traill, that the less he had to do with plates made for ordinary commerce, where there was a considerable bickering about the price, the better were the results obtained. The employment of those plates would entail considerable difficulty in the course of Mr. Fox.

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Mr. Fox. the work. With the finest pig metal used in a careful way the best results were secured, but the other course would entail loss.

Mr. Milton. Mr. J. T. MILTON said the Author had asked him to accompany him that morning to see the effect of blue heat on some mild basic-steel angles. The experiments showed that a piece cut cold from the angles stood, in the way he had already described, thirty-one bends before breaking. A piece cut from the same angle drawn out cold stood the same number, thirty-one, and when drawn out red-hot it stood forty-one. The pieces which had been drawn out at a blue heat broke at the first bend when tried cold, while a third one stood fifteen bends; when bent at a blue heat, one piece stood two and a half bends, one three bends, and another four bends. The effect of working steel at that heat, was that what was ordinarily called ductility was entirely taken away from the metal. In his opinion the temper-test was a very important one. A few months ago he had made some experiments on different qualities of steel, with reference to that very point. In the case of very mild steel of 25 tons original tensile-strength, when made red-hot and quenched the strength was raised to 38 tons per square inch. Whereas it originally stretched 24 per cent. in 10 inches, after quenching it only stretched 10 per cent. In the case of harder steel of 29 tons strength, after the quenching the strength went up to 43 tons, and the ductility diminished from 23 per cent. to 5 per cent. Still harder steel of 35 tons tensile strength, which when annealed had an elongation in 7 inches of 22 per cent., when quenched went up to 50 tons, with no elongation whatever. The quenching was done in water at a temperature of 80°. When quenched in soapy water, the hard Siemens steel, instead of standing 50 tons, only stood 43 tons, but still it left 11 per cent. extension; so that after quenching in soapy water it would no doubt stand considerable bending, whereas by quenching in cold water it would not do so. Mr. West had stated, in reference to the unexplained manner in which some steel boilers had failed, that he thought there must be a personal element in the testing of the original plates, because he had found that the tests made after the failure did not agree with the original tests. The original strength of the steel was between 26 and 29 tons, but after the failure it ranged from 24 to 40 tons. Mr. Milton thought it possible that either in the making of the boilers, or in their use, or in something which had happened to them from the time when the steel was delivered until the lapse of the two and a-half years in which they were in use, the part of the plate that showed a strength of 40 tons might have

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undergone treatment somewhat analogous to what was called the tempering test, being made hot and suddenly cooled, which would fully account for the 40 tons without throwing any stigma upon the tester. It had also been stated that while the steel in question was being tested before it left the works there were many rejections. That showed that the tester was up to his work, and doing it properly. Mr. Milton.

It ought to be clearly remembered that the strength of steel, as shown by test-pieces, was not the useful strength of the material, it was not the strength which might be expected to be got in the structure. It might be said that steel had an elongation of 24 per cent., but the useful strength was attained, as Mr. Baker had said, when the elongation was only $\frac{1}{8}$th of 1 per cent., that was, when it reached its elastic limit. In his experiments, some very mild basic steel of an original strength of 25 tons had an elastic limit of 17 tons only, and that was the useful strength of the material when put into a structure. Steel of 29 tons strength had an elastic limit of 18 tons, and 35-ton steel had only an elastic limit of 20 tons, so that in going up from 25 tons to 35, the increase in usefulness was only from 17 to 20. He thought it was doubtful whether, in such intricate forms as existed in boilers, and forms that were generally acknowledged to require a great deal of ductility in the material, it was desirable to insist on these high strengths, seeing that so little was gained by it, and that questions arose referring to hardening and tempering, which were absent in the case of mild steel.

Mr. W. Parker observed that it had been intimated to him by one of the members of Council that, in order to further clear up this question of steel becoming brittle when worked at a blue heat, it was desirable that some experiments should be made on a more extensive scale; and he had taken the matter in hand as far as time would admit, and had brought with him some pieces of steel which he had been testing with that object in view. The piece of plate which he tested was of a mild description, and of the best quality that could be produced, for the inside of a boiler. It was 4 feet long by 3 feet 6 inches wide by $\frac{7}{16}$ inch thick, and he had had it cut into three pieces. Those pieces were first of all put into an annealing furnace and annealed, in order to remove any injury that might have been caused by shearing the edges. The first piece had four strips cut from it. The first of these strips was bent cold to an angle of 45°, and then reversed and bent again to the same angle eight successive times (four one way, and four the other) without any appearance of Mr. Parker.

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Mr. Parker. fracture. The next strip was made blue-hot, and in that state it was bent in the same manner, but it only stood three and a half bends before breaking. The next piece, number 3, was treated in like manner, and it only bent two and a half times before breaking. Piece number 4 was heated to a blue heat, and then hammered with a light steam-hammer in order to put some work upon it at that heat, and it bent four times before breaking. It was allowed to get cold before bending, so that there could be no doubt as to the cause of the injury being the work put on it at blue heat. Those tests to a certain extent corroborated the experiments made by the Author, but he thought he would go a little further, and try the effect of work being put upon the other two pieces of plate in the same manner as would occur in ordinary practice. The second piece of plate, numbered 5 in the sketch, was made blue-hot and passed through ordinary boiler-makers' rolls until it was bent to the curvature shown by the specimen. It was bent to almost a circle, about 19 inches in diameter. It was then put back into the rolls again, and bent to the same curvature the reverse way, and there was no sign of fracture observable. The other piece (No. 6) was bent in like manner to the curve shown. The rolls were not small enough, nor close enough together, so that the piece could not be bent perfectly round. It was then turned the other way and bent to the curve shown, and there was no sign of fracture. Both the specimens were bent at a blue heat. Those experiments went some little way to show that, in ordinary practice, the material was not injured to the extent that it had been injured in the smaller experiments. The work put upon the small strips was of a much more severe nature than the work that had been put upon the broader plates in the same condition. In his opinion steel was injured when work was put upon it at a blue heat, but the extent of the injury all depended upon the amount of work put upon it. He should like to see this matter thoroughly cleared up, and he therefore proposed to make some further experiments, with a view of ascertaining the amount of injury done, with a certain amount of work.

Mr. John Head. Mr. JOHN HEAD said that the Author had referred to some mysterious failures in steel, and upon that point he wished to make a few observations. In his experience in steel manufacture, which had been somewhat extensive, he had found that up to the end of 1883 it was impossible to make an ingot of steel that should be absolutely free from blow-holes. He would ask the members to assume for the present that such was the case, and

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that the blow-holes would not be distributed uniformly in each ingot. When the ingots were rolled into plates the blow-holes still existed, although compressed, and they formed points of weakness in the plates. Owing to that cause sometimes test-pieces taken from the same plate would support different tensile strains. All of them would stand the same twisting and bending test, but the elongation might be different. In the tests mentioned by Mr. Baker some of the strips were found to support 24 tons, others 28 tons, and others 31 tons per square inch. He accounted for that by supposing that they had been cut from plates containing more or fewer blow-holes. There were several cases in which boiler-plates had been found to break in a mysterious manner, which he accounted for by supposing that the blow-holes had been distributed in a line in the plates. When such plates were riveted at the ends in a line with the blow-holes, of course the tensile-strength would not be materially affected, but when riveted at the sides, crossways, a certain stress would be produced which would lead to breaking. He might be told that fracture had not occurred when the rivets were put in, but only after the men had left the work. That he believed was usually the case. He would point out that when the rivets were put in they were in a heated condition, and heated the plate, thereby causing a certain amount of expansion which was sufficient to allow for the stress, but when cooling took place, stress was put upon the plate, and it snapped across in the line of blow-holes, but not necessarily through any rivet-holes. That blow-holes existed in steel could be proved if not from steel itself, at all events by means of glass. He had with him bottles which had been made some years ago of ordinary glass, and the mark of blow-holes, technically called "seedy boil," could be seen in them. The same thing occurred in steel ingots. In superior qualities of glass, like plate glass, the "seedy boil" was got rid of. After the glass was melted it was allowed to rest without flame in the furnace for three or four hours before it was poured, and during that time the "seedy boil" disappeared. It was of course impossible to deal with steel in that way, and the holes therefore must remain in all steel melted with contact of flame. Those holes were due, as had been proved in glass-making, to the flame striking on the metal, as thereby a certain amount of gaseous matter got into the glass; and it was the same with steel. In steel-making there were what were called "boily" ingots. If the steel were homogeneous and free from blow-holes, instead of boiling at the top of the mould, which necessitated stoppering with sand and wedge-plates, it should on cooling do what the men called "pipe"

Mr. John Head.

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Mr. John Head. —that was, actually contract to form a hollow at the top, but it was seldom that it did that, unless melted without contact of flame. He had been led to the consideration of the subject by Mr. Frederick Siemens' mode of heating by radiation, in which there was no contact of the flame with the steel or the glass in the furnace. He exhibited some bottles that had been made in a furnace heated by radiation, and it would be seen that they were free from the defects to which he had alluded. Mr. Gilchrist had expressed the opinion that the presence of a certain amount, 0·05 per cent., of phosphorus in steel was rather favourable than otherwise. Sir William Siemens had made some experiments on that subject in the year 1868, and Mr. Head shared in them. They made some steel containing a certain amount of phosphorus, and they found that it broke, as it was called, cold short. If a bar of steel containing phosphorus were indented with a chisel and broken on an anvil, instead of following that line, which should be the line of least resistance, it would probably break off only partly along that line or altogether along some other part; so that he was disposed to think that phosphorus would not be a desirable ingredient to import into steel.

Mr. Stroudley. Mr. W. STROUDLEY wished to give the results of some experiments made, at the request of a member of the Council, with some samples of Siemens-Martin steel, such as he used in the construction of locomotives, and specimens of which he submitted:—

First Series.—No. 1 was a piece of plate 5 inches in width, $\frac{5}{16}$ inch thick, made to his specification, from steel of 28 to 30 tons tensile-strength; it was bent to an angle of 45° over the edge of an anvil, in the way described by Mr. Parker; and it was then bent the reverse way, through an angle of 90°; and this was repeated eleven times (making a total of twenty-three times bending through an angle of 45°). It had not quite broken asunder; the fracture, instead of taking place at the edge and breaking inwards, as might have been expected, was more in the centre, the edges remaining intact. This piece was taken as it came from the manufacturer, and was bent cold. No. 2, another piece of the same plate, had been heated to a blue heat, and bent to an angle of 45° once, and then bent the reverse way through an angle of 90°. After cooling it was again bent; but it would only bear bending ten more times as compared with twenty-three times of the cold plate that had not been interfered with. The fracture extended to about the same point, the edges remaining whole as in the other specimen. No. 3, another piece cut from the same plate, was heated to redness, and bent once to an angle of 45°, and

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Mr. Stroudley.

again through an angle of 90°, in precisely the same manner. It was then allowed to cool, when it bore twenty-three additional bendings through an angle of 45°, the fracture being somewhat less than in the other instances, showing that that treatment had made it stronger than in the case of the steel which had been bent when cold, or at a blue heat.

Second Series.—Other specimens were taken to test the tensile value of this material. No. 1, a piece of steel in its original form as delivered from the manufacturer, broke at a tensile-strain of 30·5 tons, with an extension of $\frac{7}{16}$ per inch. No. 2, after being heated to a blue heat, was allowed to cool, and was then tested. It broke at 29·4 tons, showing an elongation of $\frac{5}{16}$ per inch. No. 3 was bent once through an angle of 45°, and was then straightened by hammering; when tested it broke at 33 tons, showing an elongation of $\frac{3}{32}$ per inch. No. 4 was heated to a blue heat, and bent through an angle of 45°, then straightened and allowed to cool, and was afterwards tested, showing a strength of 32·2 tons, and an elongation of $\frac{1}{32}$ per inch only. No. 5 was heated to a red heat, and allowed to cool, and was then tested. It broke at 29·4 tons, showing an elongation of $\frac{3}{8}$-inch. No. 6 was heated to redness, bent to 45°, and straightened; and when cold was tested, showing a breaking value of 29·8 tons, and an elongation of $\frac{5}{16}$ to the inch.

Third Series.—Pieces of steel, 14 inches long by 4 inches wide by 1 inch thick, were cut from framing-plates, the edges of the plate being planed smooth. No. 1 was bent whilst cold to an angle of 45°, then laid aside to cool from the temperature caused by the first bending; it was then bent through another 45°, or equal to 90°; again laid aside to cool as before, then bent another 45°, or equal to 135°; cooled as before, and finally folded close without fracture. No. 2 was bent whilst at a blue heat, to an angle of 45°, and allowed to cool; then bent to an angle of 90°; then to 135°, and finally to within $\frac{3}{16}$ inch of being folded over close when it broke on the inside edge of one fold, the fracture being $\frac{1}{8}$ inch open. This specimen was also allowed to cool between each bending. No. 3 was bent whilst at a red heat to an angle of 45°, and allowed to cool; it was then bent to an angle of 90°, then to 135°, and finally folded close when it was slightly fractured on the inside edge of the plate. No. 4 was heated to a bright red and allowed to cool. This was folded close, and was slightly fractured on the inside of the plate. No. 5 was bent while at a blue heat to an angle of 90°; then made a bright red, put into hot ashes and allowed to remain until cold; it was afterwards

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Mr. Stroudley. folded close to within $\frac{3}{32}$ inch, when it fractured on both sides. No. 6. Two plates were bent at a blue heat to an angle of 90°; then made a bright red and placed aside in a cold atmosphere; they cooled rather quickly, and were afterwards folded close without fracture. No. 7 was bent while at a blue heat to 90°; then made a bright red and put into boiling water. When it had cooled to about 200°, it was placed in a pail of hot water, and the pail put into a tank of cold water. It was folded over when cold, quite close without fracture.

Fourth Series.—The specimens submitted to these tests were cut about 8 inches square, from Siemens-Martin steel frame-plate, 1 inch thick; the edges were planed smooth. In each corner a hole 1 inch in diameter was drilled, the centre of which was $1\frac{3}{4}$ inch from each edge. These holes were drifted out with drifts having $\frac{3}{64}$-inch taper per inch of length. No. 1 specimen, as from the manufacturer, was drifted whilst cold until it fractured; one hole being increased from 1 inch to $1\frac{5}{16}$ inch in diameter, and the other hole to $2\frac{1}{16}$ inches. No. 2 specimen was as before, but tested to a blue heat, the holes being drifted to $1\frac{1}{2}$ inch in diameter. It was then laid aside to cool, and afterwards tested; when opened only $\frac{1}{8}$ inch more the specimen split across, showing very brittle fracture. Other experiments were made in a similar manner, the fractures occurring at $1\frac{9}{16}$ inch, $1\frac{13}{16}$ inch, $1\frac{9}{16}$ inch, $1\frac{33}{64}$ inch, and $1\frac{5}{8}$ inch diameter of holes. No. 3, as before, was raised to a red heat, and the holes were drifted to $1\frac{1}{2}$ inch in diameter. It was then allowed to cool, and was afterwards drifted until one hole fractured to the extent of $\frac{1}{16}$ inch open, it being $2\frac{1}{2}$ inches in diameter; the other hole was drifted to the same diameter without fracture. No. 4, as before, was raised to a bright red heat, and allowed to cool. It was then drifted until fractured; one hole was $2\frac{7}{32}$ inches in diameter, the fracture was $\frac{1}{8}$ inch open. The other hole was $2\frac{3}{8}$ inches in diameter, and the fracture $\frac{1}{16}$ inch open. No. 5 was drifted at a blue heat until it fractured. The diameter of the hole was $1\frac{29}{32}$ inch; the fracture was $\frac{3}{64}$ inch open. No. 6 was drifted at a blue heat to $1\frac{1}{2}$ inch diameter, then made a bright red, placed in hot ashes, and allowed to cool. It was afterwards drifted until fracture occurred, one hole being $2\frac{5}{16}$ inches in diameter, with the fracture $\frac{3}{64}$ inch open, and the other hole $2\frac{3}{32}$ inches in diameter, with the fracture $\frac{1}{16}$ inch open. No. 7. Two plates were drifted at a blue heat to $1\frac{1}{2}$ inch in diameter, and then made bright red. Being placed aside in a cold atmosphere they cooled rather quickly. They were afterwards drifted until fracture occurred, the holes being respectively $2\frac{11}{32}$ inches in diameter,

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the fracture $\frac{1}{8}$ inch open, and $2\frac{9}{32}$ inches in diameter, and the fracture $\frac{3}{64}$ open; and $2\frac{1}{2}$ inches in diameter not fractured; and $2\frac{11}{32}$ inches in diameter, with the fracture $\frac{3}{64}$ inch open. No. 8 was drifted at a blue heat to $1\frac{1}{2}$ inch; then made a bright red and put into boiling water. When it had cooled to about 200°, it was placed in a pail of hot water and the pail put into a tank of cold water. One hole was then drifted cold, to $2\frac{1}{2}$ inches diameter, and was not fractured; the other hole was fractured at $2\frac{7}{16}$ inches diameter. No. 9 was drifted at a blue heat to $1\frac{1}{2}$ inch diameter, then made a bright red and put into cold water. One hole only was drifted when cold to $2\frac{1}{2}$ inches diameter. Mr. Stroudley.

Fifth Series.—These tests were made with pieces of Siemens-Martin steel, sheared from frame-plates, having a 1-inch hole drilled in each corner, the centres of the holes were $1\frac{3}{4}$ inch from the nearest edges. These plates were sheared with ordinary scrap shears. No. 1 specimen was drifted whilst cold until it fractured. The first hole was $1\frac{9}{32}$ inch in diameter, the next $1\frac{5}{32}$ inch, the third $1\frac{7}{16}$ inch, and the fourth $1\frac{9}{32}$ inch in diameter. No. 2 was drifted at a blue heat to $1\frac{1}{2}$ inch and allowed to cool; it was then drifted to $1\frac{3}{4}$ inch in diameter, when it fractured, the fracture being $\frac{1}{16}$ inch open. No. 3 was drifted at a red heat to $1\frac{1}{2}$ inch and allowed to cool. It was then drifted until fractured, one hole being $2\frac{1}{4}$ inches in diameter, with the fracture $\frac{1}{8}$ inch open, the other being $2\frac{3}{8}$ inches in diameter, with the fracture $\frac{1}{16}$ inch open on the inside of the hole. This occurred in this case only. No. 4, after being raised to a bright red heat, was allowed to cool, and then drifted until it fractured. One hole was $1\frac{15}{16}$ inch in diameter, the fracture being $\frac{1}{8}$ inch open, and the other hole $2\frac{1}{16}$ inches in diameter, the fracture being $\frac{1}{16}$ inch open.

With a view to determine the injury, if any, caused by working Siemens-Martin steel of 28 to 30 tons tensile-strength, such as would be used for marine-boilers, engine frame-plates, &c., several tests were made, which were thus arranged:—1. A specimen plate, as delivered from the maker, was tested in each of the several series, in its natural form. 2. Another specimen from the same plate was tested whilst at a blue heat. 3. Another specimen, after having work put upon it, was tested at a blue heat. 4. Another specimen was tested at a red heat to one-half the test, the finish being whilst cold. 5. Another specimen was tested after being heated to redness, and allowed to cool, so as to anneal it.

The behaviour under these various conditions of bending, testing for tensile strength, and spreading out by punching a drilled

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Mr. Stoudley. hole to about double its diameter, went far to show that the views of the Author, that the elasticity of the steel was permanently injured by being worked at a blue heat, whilst it was not injured by being worked cold or at a red heat, were correct. It would also be noticed that the steel was not injured by being heated to a bright red and plunged either into hot or into cold water. He must, however, direct attention to the fact that the specimens which were tested in this case were of the very highest quality, by the best makers in Yorkshire. The results of his experiments, as far as he could judge, supported the idea that working steel at a blue heat did permanently injure it. Hitherto he had been of opinion that it was best and safest to work at a blue heat; but he was now satisfied that it was not so, and he thought that great credit was due to the Author for having brought the matter forward.

Mr. Hodge. Mr. W. R. HODGE had been for nearly forty years engaged in the practical operation of making boilers and similar structures, and therefore took great interest in the subject of the Paper. He had been particularly struck with the statement that blue heat was injurious to the working of Lowmoor or superior Yorkshire iron. For years the practice with boiler-makers had been to put a large piece of red-hot iron on plates requiring closing, and heat them to a temperature that was now considered dangerous, about 600° Fahrenheit, and during all his experience he had never seen any very serious consequences result therefrom. As the matter was one that interested all persons connected with engineering, he had resolved to try an experiment, and he accordingly cut four pieces, two from Bowling plate, and two from a steel plate, both 2 inches wide, and $\frac{5}{16}$ inch thick, and he submitted them to exactly the same treatment. They were heated to a blue heat, about 600° Fahrenheit, as far as he could tell without a gauge, and they were bent over the hammer block of a steam-hammer at right angles, then straightened at the same temperature, and then bent again and again till the samples broke. He then heated the other ends of the same pieces to blue heat, and hammered them under the same hammer, in order to flatten them to the extent of 20 per cent., so that the $\frac{5}{16}$ inch was reduced to $\frac{1}{4}$. He allowed those pieces to get cold, and twenty-four hours afterwards he submitted them to the same kind of treatment that he had given to the others. The result was that the Bowling iron went down very much in strength and value, while the steel if anything increased in strength and value after the hammering. He then tested in the same way two independent pieces, bending

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them cold, and the hammered end in the Bowling iron went down in strength and value, but the steel did not. The steel was manufactured by a well-known Scotch firm, and he thought it was of a fair average quality of Siemens steel. The firm were as good makers as any he had known. The only fault he had to find with Siemens steel was the difficulty in welding, a difficulty that still existed. In cooling incipient cracks parallel with the scarf were developed. Every boiler-maker, and every one engaged in that kind of business, could not but rejoice at the extreme homogeneousness of the material. It was now a real pleasure to execute difficult flanging, and other work of that kind, with so excellent a material as compared with iron, even of the best quality. The Author had suggested the carrying on of the experiments a little further, by bending two pieces of plate of equal quality and equal size, if possible, to breaking, one being bent cold, and the other submitted to blue-heat or black-heat treatment. He exhibited two such specimens. One specimen was heated blue, and was bent three times each way in ordinary boiler-makers' rolls, and rolled up to a 1 foot 3 inches radius, then turned over and rolled up the reverse way to a 1 foot 3 inches radius at one rolling. The other was treated in the same way cold. Subsequently, when the specimen by contact with the rolls went down to what seemed to be less than 600° Fahrenheit, he increased the heat again by putting it into the furnace, and then submitted it to nine successive bendings each way, making in all twenty-four bendings at a blue heat. He treated the cold piece in the same manner, and there were no signs of fracture in it of any kind. It appeared, therefore, that although blue heat did deteriorate steel, it did not deteriorate all steel alike. There must be considerable differences in the qualities of steel, and he thought that it was very much more in the hands of manufacturers to produce a trustworthy article, than it was in the hands of those who had to use the material. If manufacturers would address themselves to that point, he believed the problem would not be found so very difficult. His experience of the material in question did not accord with that of Mr. Jeremiah Head, who had stated that he had seen some of it almost go to pieces, as Mr. Hodge's firm had commenced making boilers with Siemens steel in 1879, and they had never known it go to pieces in that way; in fact, their experience had been all the other way, tending to establish the uniformly good and satisfactory character of the material. The only other difficulty, besides the welding, was that on very rare occasions (he only knew of two) the Siemens steel had come out

Mr. Hodge.

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Mr. Hodge. a little laminated; but its homogeneousness, as compared with Bowling or Lowmoor iron, was remarkable. In days gone by, every third or fourth plate of Lowmoor or Bowling iron when severely worked was rejected, on account of its unsoundness. He would recommend manufacturers to be particular in recording all failures, and the users of the material to tell the manufacturers of the same, so that they might address themselves to the task of finding out what was the real cause of the peculiar behaviour of some steels, and endeavour to produce a material which would be as superior to Lowmoor iron, as Lowmoor iron was to common iron.

Mr. Cowper. Mr. E. A. COWPER thought that the pieces tested by the Author were, perhaps, rather small for good practical proofs, but nevertheless, he had been led to the conclusion that blue heat was injurious as a working temperature for iron. Mr. Cowper had always bent iron either cold or hot. When iron was hot it was in a malleable state something like wax, so that it could be bent backwards and forwards without a wound, and that was forging. When it was bent cold it took a set, and if it was bent backwards and forwards often it would crack. But bending iron at blue heat was neither the one nor the other. The material was not sufficiently soft to be malleable and wax-like, but it was stiff enough to be injured by the bending. Whether it was more injured by bending at a blue heat than by bending cold was entirely a question of experiment, and the experiments recorded in the Paper seemed to point to that result. It was impossible to exaggerate the importance of the subject, because if some expressions in the Paper were to be taken as absolutely proved, engineers had all been wrong in their boiler-making and ship-building, and they could no longer treat plates in the way they now did. He alluded particularly to the intimations, on pages 114, 121, 123, and 124, to effect that "steel plates and bars have failed," and with "little doubt at a blue heat." "All these results-point unmistakably to the great danger" at a blue heat. The plates "should under no circumstances be hammered or bent" at a blue heat. It was a "question whether one quality of steel is and another is not affected by long-continued exposure to a blue heat." And, finally, it being said to be clear that the "experiments" "show how steel can with certainty be made permanently brittle." Now, it was an undoubted fact that steel bent with less force, when at a blue heat, than it did when cold. Screw pins or studs had been bent when heated by a piece of red-hot gas-pipe, if put in at a wrong angle, but he preferred to drill the holes and tap them

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truly by letting the drill and tap be guided through a block fixed truly (as was the practice with some of the best engineers) to bending them after they had been put in wrongly. Mr. Cowper did not mean to say that steel-plates were not sometimes injured if worked at a blue heat; he believed that to some extent it was the case, and it would be most interesting to investigate to what extent this was so, and under what circumstances. He preferred to work a plate or even bend it, either when cold or at a red heat, and then anneal it; but in some cases hardening to a blue temper had the most beneficial effect in increasing the toughness of the steel, and at the same time its strength, to a marvellous extent. For hair-springs of watches and chronometers, and for coach-springs of all kinds, the tempering to a blue-temper, was the best treatment to which they could be subjected. A hair-spring often made eight thousand million vibrations in a lifetime. Again in the use of hard drawn steel wire for plough ropes for steam ploughing, for pit ropes, and for piano wire, &c., the plan of hardening to blue temper, and then drawing, increased the toughness of the wire to a great extent, and the tensile strength from 34 tons per square inch to 75 tons, and if it was made still harder, and drawn, it took 108 tons per square inch to break it. The "Austrian music wire," that entirely commanded the market here for some years, was made in this manner, and an English firm having adopted the same plan, realized a large fortune, and now it was a common process. There were a few observations in the Paper, as to different qualities of steel standing different numbers of bends; but there was no specification of what those qualities were, and no analysis whatever, nor was the temperature of the "core-store" given. He thought that in an inquiry such as this, exact analyses should be given, and he begged the Author to supply them. The Author stated, for instance, that one piece of iron only stood a very few bends red-hot, and there could not be a doubt but that it was decidedly "red short," and probably contained sulphur. He once was supplied with some thousands of spokes, about 3 inches by $\frac{5}{8}$, so utterly "red short," that they would not bend when red-hot to a 2-inch radius, in fact they were utterly worthless at a red heat, and yet they would bend nearly double when cold. The qualities of iron were very various, as for instance in "boat-plates" (a miserable quality), "common iron," "Lowmoor," "cable iron," "Marshall iron," &c., &c.; and the difference in their working was far more than was experienced in bending iron when hot, or when at a blue heat. The experiments of the Franklin Institute were well known, though they were rather old now; but it was

Mr. Cowper.

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Mr. Cowper. then found that iron was stronger for a tensile strain, when kept hot in a melted lead bath.

Some years ago, before steel was made at Middlesbrough, iron so often contained a considerable proportion of phosphorus as to be very "cold short," and it was nick-named "Middlesbrough steel;" hæmatite had to be used with it, to make a good bottom flange in tension, to stand certain tests applied to single-headed rails. He was extremely obliged to those members, who had taken up the idea of making practical experiments on a sufficient scale, as to the extent of the injury sustained from bending a mild steel plate at a blue heat, and for the samples of plates exhibited, that had been bent backwards and forwards at a blue heat. Some of these samples showed no symptoms whatever of cracks, or injury, while at the same time, others indicated that some injury had been sustained in certain cases, and it was generally agreed that it was best not to work a plate much at a blue heat. He quite admitted that the mildest quality of steel plates should be used for the purposes of tension, in all large boilers carrying heavy pressure, and that it should not be attempted to get strength by adopting harder steel, with more carbon in it, although pieces of the latter would stand a higher tensile strain when proved in a testing machine. Reference has been made to some of the steel used in the Forth Bridge being of 34 and 37 tons tensile strength. That steel, however, was all used in compression, not in tension. Mr. Baker wished him to mention this, and he believed he would not think of using such steel in tension, because it was too brittle for that purpose. For boiler-plates steel could hardly be too mild, a tensile strength of 25 or 26 tons per square inch was enough; certainly, the strength should be under 30 tons. In all cases of tension it was advisable to have tough soft steel with great malleability; steel with a higher tensile strength might be used in certain situations where it was entirely in compression.

Dr. Pole. Dr. W. Pole, Honorary Secretary, said that as the discussion had diverged to a certain extent into the mysterious properties of steel generally, the members might like to know a fact that had been lately communicated to him by Dr. Percy, as to the extraordinary qualities shown by steel in the form of wire. Dr. Pole had occasion in 1862 to make some experiments on the strain of pianofortes; and he found that the steel wire used for them showed a most remarkable tensile-strength. It was well known that if wire of a certain length and a certain weight sounded a certain note (giving a certain number of vibrations per second), it must be subject to a certain tension; and testing in that way it

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was found that the pianoforte wire stood a strain of from 100 tons to 120 tons per square inch. He had mentioned the fact at this Institution,[1] but at that time no one had ever heard of such a strain being withstood by steel in any form, and the assertion was doubted, even by no less an authority than Sir Joseph Whitworth. It was, however, tested and found to be true. Dr. Percy, who had taken a great deal of interest in the matter, had recently told him that he had some steel wire which was actually standing, as tested in the machine at Woolwich with great care, the extraordinary strain of 154 tons per square inch, and he hoped that Dr. Percy would publish the particulars. In corroboration he had heard from Messrs. Broadwood that during the last ten or twenty years they had found that pianoforte wire had been greatly increasing in strength. It was a remarkable thing that steel in that form should possess such extraordinary and almost unheard-of tenacity. He ought to add that the wire alluded to was by no means hard and brittle, as steel of very high tensile power often was. It was subjected in its use to severe usage. It was wrapped round a small pin which served for tuning, and it was turned into loops and subjected to twists, showing that it had a certain amount of ductility. The strings also received violent blows, which, however, appeared to do them no damage. Dr. Pole also thought it right to mention another remarkable quality shown by steel wire, namely, the very high range of its elastic limit. A pianoforte string was, when in use, strained almost to its breaking point, and yet the elasticity remained perfect, as was shown by the fact that the string would, when once tuned, stand in tune, *i.e.*, would receive no permanent elongation, for an indefinite length of time. Dr. Pole conceived that a form of the metal which showed these extraordinary qualities was well worth study, and that if the conditions which produced these qualities could be exactly determined, the investigation might throw more light on the properties of steel generally. Dr. Pole.

Mr. James Mansergh stated that he had frequent occasion to erect steam-boilers, and until recently had always specified iron, on account of the more or less serious failures that now and then occurred with steel. Gradually, however, the consensus of opinion appeared to be inclining decidedly towards the adoption of steel, and a year ago he ordered three large Lancashire boilers of that material, which were just ready to be delivered when the alarming report by Mr. Maginnis on the failure of two marine-boilers Mr. Mansergh.

[1] Minutes of Proceedings Inst. C.E. vol. xxi. p. 242; vol. xlii. p. 102.

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Mr. Mansergh. appeared in "The Engineer." That report was a most disquieting one, as the steel plates of which the boilers were made had passed all the Board of Trade and Lloyd's tests, and stood without the slightest defect the ordinary work of the boiler-shop, welding, etc., and their collapse after two years' use appeared inexplicable. The Paper under discussion had come most opportunely, and its main point, the danger of working at a blue heat, might he thought account for these and other failures. He had obtained the views and experience of the successful makers of the three boilers referred to, which might be as interesting and comforting to other users of steel-boilers as they had been to himself. This firm had during the last six years made upwards of two hundred steel boilers, principally of large size, and in the last three years a proportion of twenty steel boilers to one of iron. In the first and second years they had several slight mishaps with Sheffield plates, by flue-tubes cracking at the roots of the flanges during cooling; but since then they had not experienced a failure of any description. He had enquired specially as to their practice in working, and found that they heated the steel plates up to very nearly the white heat customary with iron, and ceased working as soon as it came down to a dull red, considering it dangerous to approach the blue heat referred to in the Paper. The rule-of-thumb test suggested on page 121 of the Paper, by drawing a stick or hammer handle over the blue plate, and not ceasing to work so long as the mark glowed, was considered unsatisfactory by them, as this glow would appear when the steel had become much too cold to work with safety. In their opinion it was a dangerous rule to approve of. Probably the absence of failure in their boiler-making during the last four years might result from the precautions they had taken in this respect.

Sir E. J. Reed. Sir E. J. Reed, K.C.B., M.P., thought the discussion had been a very valuable one, and full of interest to all, and to none more than to those whose experience of steel had been that it was incomparably superior to iron, but that, nevertheless, it had occasionally proved to be a material which it was rather difficult to understand. In shipbuilding he did not think there was so much trouble as in boiler-making; but in the establishments in which steel was first very largely used for ship-building, Her Majesty's dockyard officers and men became perfectly enamoured of it as compared with iron, finding it much more trustworthy in every way, and much more capable of being dealt with reliably. Of course that did not apply to steel in the form in which it was known at first. He remembered a very striking instance of its treachery; some steel had been put

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into a stringer-plate in a ship, and it had been worked into place apparently very satisfactorily, but on looking the following morning, he found that it had cracked during the night, not through the rivet-holes or any point of weakness, but through the solid plate, owing solely to a change of temperature. But when the makers were limited to a low maximum of tensile strength, and produced the material now in general use, it was found to be of such a character that shipbuilders would be exceedingly sorry to lose it. During the last three months he had closely observed the results of tests in steel manufactured for certain vessels under his care in six or eight manufacturing establishments, and in no case had it been necessary to reject a plate for its want of ability to conform to the rather severe Admiralty tests. In fact, the introduction of steel had saved shipbuilders nine-tenths of the trouble they used to experience in getting a satisfactory material, when they had to look for the best iron for a tensile strength of 22 tons per square inch, in conjunction with other good qualities. But the interest of the Paper lay chiefly in the facts that had been developed, not when raising or lowering steel to a blue heat, but when putting work upon it in addition, and those facts had thrown a good deal of light on many of the incidents which shipbuilders had had to observe. Generally speaking, so much satisfactory experience had been had with good mild steel in shipbuilding that most of the bending had been done with the steel cold, no trouble being taken to heat it. It was extraordinary what an amount of shaping and handling it would take when cold, without apparently undergoing any deterioration. But it was not an uncommon practice to heat steel moderately, and up to what proved now to be an unsatisfactory blue heat, and while at that heat to put work upon it. There had been instances of comparative failure, and he believed that if they were examined it would be found that they grouped themselves around that condition of working with the blue heat. It was for that reason that he thought the Paper would be particularly valuable to shipbuilders, and every wise man engaged in the profession would pay careful attention to the Paper, and to the discussion upon it—one of the most valuable he had heard on a technical matter. One point had not been noticed in connection with the many interesting and instructive experiments to which reference had been made. Statistics had been given of the angles to which specimens had been bent, and the number of times which they had been bent; but nothing had been said as to the degree of rapidity with which the change of form was brought about, and that, he

Sir E. J. Reed.

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Sir E. J. Reed. thought, was a matter of considerable importance. A piece of plate might be subjected to those alternate bendings to a given amount, and yet afford very different results, according to the degree of rapidity with which the changes of form were effected. He agreed that it would add materially to the educative, and, indeed, to the general value of the experiments which so many gentlemen had taken the trouble to make, if they would furnish full information about the material with which they had worked. Nothing was known of the chemical constituents of any of the materials used, and those who had listened to Sir Frederick Abel's speech, must have been very much struck with the necessity of being acquainted, not only with the quantity, but also with the form in which the carbon entered into the structure of the material. The debate suggested the desirability of an elaborate course of experiments on the question. He thought there was no department of shipbuilding or of boiler-making but would be benefited by a more complete series of experiments, attended by fuller information as to the material operated upon. He was sorry that he was unable at present to add any specific facts bearing upon the question. He had, however, derived a great deal of valuable instruction from the discussion.

Mr. Stromeyer. Mr. STROMEYER, in reply, said he had tried to make his Paper as short as possible, by touching only one subject, though of course, from his experience in going through boilers almost every day, he could have cited instances that had come under his notice which would have added interest to a discussion, on the mysteries of steel failures, but he had thought it best to adhere as closely as possible to but one subject. As, however, the case of the Maginnis boiler had been mentioned, he might be permitted to refer to some failures in iron boilers that had come under his notice which closely resembled them. It was part of his duty to examine marine-boilers at various periods of their life, and having been requested to examine a pair of iron ones which had been built in 1878, according to the sketch (Fig. 12), and which had been out of use for about three months, he found that the back of the combustion-chamber had cracked 23 inches in the way shown in Fig. 13. In his mind there was no doubt that the failure had occurred within twelve hours previous to the survey. As the crack extended beyond a seam it could not have been due solely to bad quality of iron. In fact, the iron appeared soft enough when chipped and drilled. That appeared to him to be quite as mysterious as any failure that had occurred with steel boilers. He knew of another case in the sister ship to the one he had men-

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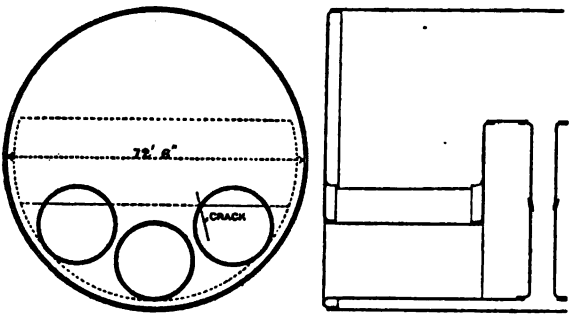
tioned where the top plate cracked, and also another where a crack occurred as shown in Fig. 14. This one cracked during the Mr. Stromeyer.

Fig. 12.



dinner hour with a loud report about two weeks after the boilers were blown down. In the first of these cases the iron seemed perfectly good, so that there was a great amount of mystery

Fig. 13.

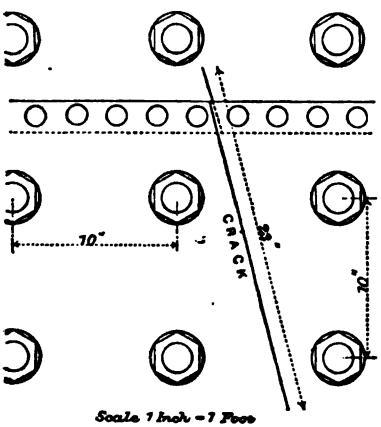
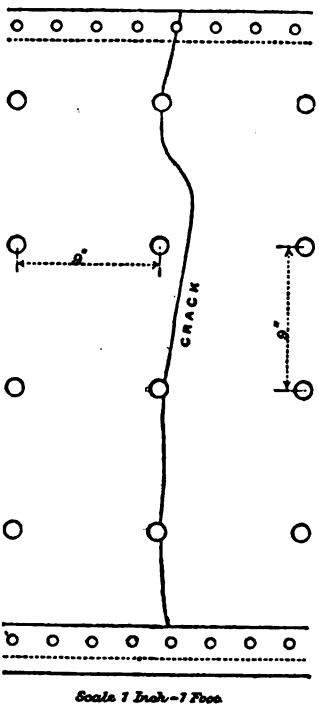


Fig. 14.



attached to it. He did not think the steel boilers were any worse

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Mr. Stromeyer. than iron ones. It would be noticed that in all these instances the backs of the combustion-chamber were flat and very large, as they were also in the boilers reported on by Mr. Maginnis, which would point to this form of construction being faulty rather than to the material being bad. Doubtless the steel, of which it was known that 40 per cent. had been rejected, and possibly the iron, were not as good as they ought to have been; and probably if the material of all these boilers had been of the highest quality, even a more serious fault in the construction would not have produced such effects. It was not at all the object of the Paper to explain such mysteries, but to show that steel and iron could be spoiled by a certain treatment. A favourite theory was that the strains set up by local heating caused subsequent failures of steel. It was said that if the corner of the plate was heated, the unequal contraction of the plate would cause strains which would lead to breaking; but if the plates would stand 25 per cent. elongation in the testing-machine, he could not see how they could crack. If any strains were set up, the 25 per cent. elongation would allow them to ease themselves. But for all that there could be no doubt that cracks did occur. He remembered that some years ago a garboard plate had cracked in a very mysterious manner; he investigated it, and found that by putting work on it at a blue heat he could permanently injure it. This was the commencement of the experiments submitted in the Paper. One of his objects had been to discover the method of producing similar failures in test-pieces. He had not experimented with a view of showing whether the injury could be removed by annealing, but he had no doubt that this could be done. As an instance of how narrow the limit was within which steel was spoiled, he would refer to Mr. Milton's experiments. Three test-pieces were prepared by hammering them hot under a steam-hammer. Two of them broke with one blow of a hammer, while the other one stood fifteen bends. They were all prepared in the same manner, though one might have been a little warmer or a little colder than the others, or the hammering might not have been so severe. Whatever the differences of manipulation might have been they were certainly so small that the great difference in the result was very unexpected. He had tried to show how to spoil steel, in order to warn every one against approaching that point. He had hoped that manufacturers of steel would be induced to make many more experiments, but he doubted whether curving large plates would give useful information. Curving was not as severe as the bending small test-pieces, and had led, as was shown by Mr. Hodge and Mr. Parker,

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to no results. It was evidently very difficult to spoil steel by workshop treatment, for many thousands of boilers had been made from it, and many plates must have been treated at a blue heat, yet mysterious failures were exceedingly rare. From the specimens he had shown, it would be seen that steel worked at a blue heat would stand two or three bends; this was even more than good iron would stand when it was cold. If he might suggest the direction in which further experiments should be carried out, he would propose that manufacturers should make about a dozen bending tests under various conditions on all steel charges of which they had made a complete chemical analysis. A comparison of numerous results might show the influence of the various impurities on the blue-shortness of steel, and might point to a method of removing it. Another question, though of less importance, would then be to ascertain at what temperature the various steels would stand most work with the least amount of permanent injury. He then gave the following explanation of hardness, brittleness, ductility and softness. The samples to which he had referred when broken at a blue heat could not be called brittle; he should rather call them rotten, they broke quite suddenly, and still they had silky fractures and not crystalline ones; but if worked at a blue heat and then allowed to rest, and then broken cold, they had crystalline fractures. Steel that had been prepared in this manner could be called brittle, but not hard, for it could easily be filed. Hardened tool-steel could be equally brittle, but it was much harder. He had measured the ductility by the number of bends, probably it could also be estimated from the amount of elongation or contraction of a test-piece. The softness would have to be measured by the amount of work required to bend the samples or to tear a test-piece. It was found to vary considerably in annealed and quenched mild steel, though the ductility was the same in both cases. Professor Unwin had stated that the stress-diagrams, given in the Paper, might be incorrect by about 5 per cent., but as he had made all the measurements with an instrument that measured to the $\frac{1}{10,000}$ inch, he doubted whether the errors could be even $\frac{1}{4}$ per cent. From measurements which he had made on the density of steel, before and after testing, he felt sure that it altered very slightly. He had not discussed any of the tensile-tests for fear of lengthening the Paper and prolonging the discussion on matters which did not properly belong to the subject. He had chiefly referred to the bending-tests, as they were the most practical ones. The other tests, however, were quite worthy of study. There was far more in the Tables in-

Mr. Stromeyer.

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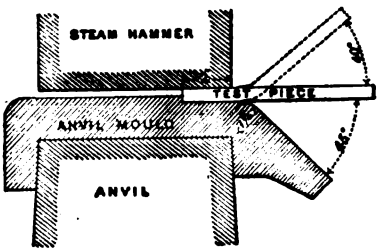
Mr. Stromeyer. cluding the bending-tests and the diagrams than he had been able to explain, and if they were studied many points would suggest themselves which could be further experimented upon. He hoped that one day manufacturers would be able to produce steel that could not be injured at a blue heat. As it was, steel was one of the best materials for constructive purposes, and if steel could be produced which could not be injured by blue heat there would be nothing more to be desired, and he hoped that some important result in that direction might be attained. The manner in which the test-pieces, mentioned in Tables V. to VIII. were bent would be seen from Fig. 15. An anvil mould was forged

FIG. 15.



to fit the anvil-block of a steam-hammer, and shaped so that one end projected and was inclined to an angle of 45°; the bend was rounded off to a radius of 1¼ inch. The top of the mould was made ⅛ inch higher than this radius, so that the test-pieces could butt against the ledge thus formed. When experimenting, the test-pieces were inserted as shown, and hammered down to the slant; it was, however, found that on account of the spring of the metal the bend only amounted to 40° and the radius of curvature to 1¾ inch. The test-piece was then removed and replaced, as shown by the dotted lines, and the hammering continued. It took from two to four blows with a ¼-cwt. hammer to effect one bend. The quenched pieces required most hammering.

The following were the analyses of the steel and iron:—

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MEDIUM HARD STEEL (TABLE V.).

	Test-piece.		
	No. 12.	No. 42.	Mean.
Carbon	0·294	0·292	0·293
Silicon	0·031	0·027	0·029
Manganese	0·640	0·630	0·635
Phosphorus	0·093	0·091	0·092
Sulphur	0·080	0·083	0·081
Copper	0·030	0·024	0·027

MILD STEEL (TABLE VI.).

	Test-piece.		
	No. 25.	No. 43.	Mean.
Carbon	0·162	0·155	0·158
Silicon	0·021	0·015	0·018
Manganese	0·590	0·584	0·587
Phosphorus	0·060	0·063	0·061
Sulphur	0·060	0·072	0·066
Copper	0·024	0·016	0·020

LOWMOOR IRON (TABLE VII.).

	Test-piece.			
	No. 1.	No. 14.	No. 42.	Mean.
Carbon	0·019	0·019	0·022	0·020
Silicon	0·125	0·155	0·129	0·136
Manganese	0·080	0·060	0·051	0·054
Phosphorus	0·089	0·076	0·068	0·078
Sulphur	0·009	0·007	0·004	0·007
Copper	0·040	0·027	0·030	0·032

VERY MILD STEEL (TABLE VIII.).

	Test-piece.			
	No. 4.	No. 6.	Mean.	Charge.
Carbon	0·049	0·049	0·049	0·054
Silicon	0·012	0·017	0·014	trace
Manganese	0·176	0·145	0·160	0·170
Phosphorus	0·063	0·067	0·065	0·070
Sulphur	0·043	0·037	0·040	0·080
Copper	0·096	0·100	0·098	0·060

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Mr. Stromeyer.

All the above analyses were made in one laboratory. The test-pieces which were drilled for analysis were selected from the extremities of plates used for experimenting.

An experiment, similar to those explained by Mr. Parker and Mr. Hodge, was carried out on an iron plate $\frac{3}{4}$ inch thick, 30 inches wide and 4 feet long, which was sheared into two halves lengthways. One of them was twice curved when cold, and the other one when hot (blue to light straw), to a radius of 15 inches. The hot plate broke in the rolls. Subsequently test-pieces were cut about 12 inches from one edge of the plates and thus tested.

	Curved when Cold.	Curved when Blue-hot.
Unprepared . .	Bent to $\frac{3}{4}$-inch radius . .	Broke 20° bend.
Red hot, quenched in cold water .	„ double	Bent to $\frac{3}{4}$-inch radius.

The diagrams, Figs. 16, 17, 18, 19, referred to the tests in Table I. No analysis of these steels had been made.

FIG. 16.

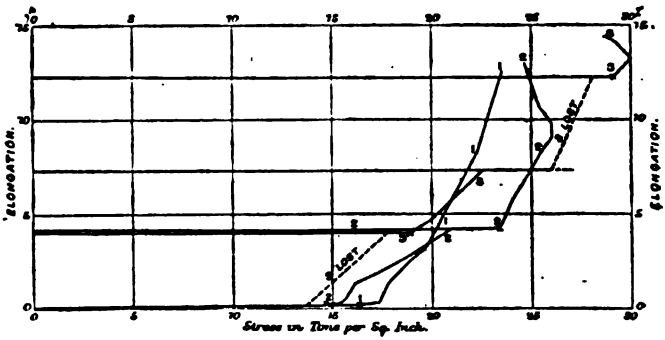
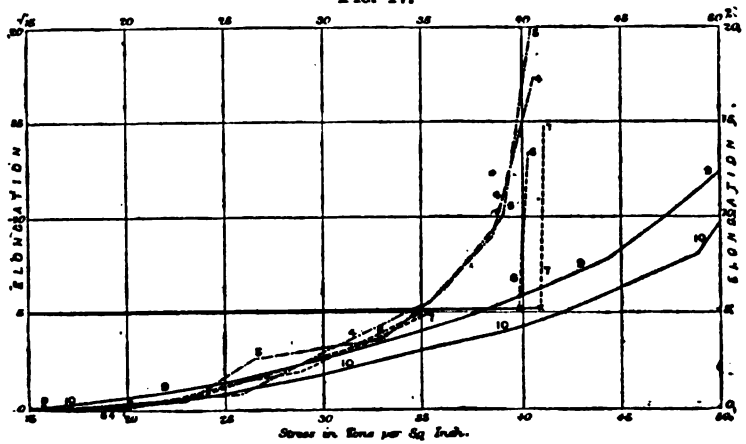


FIG. 17.



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Mr. Stromeyer.

FIG. 18.

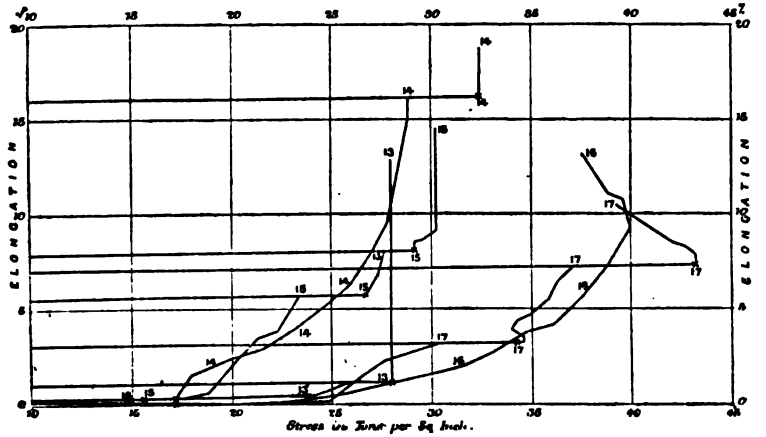
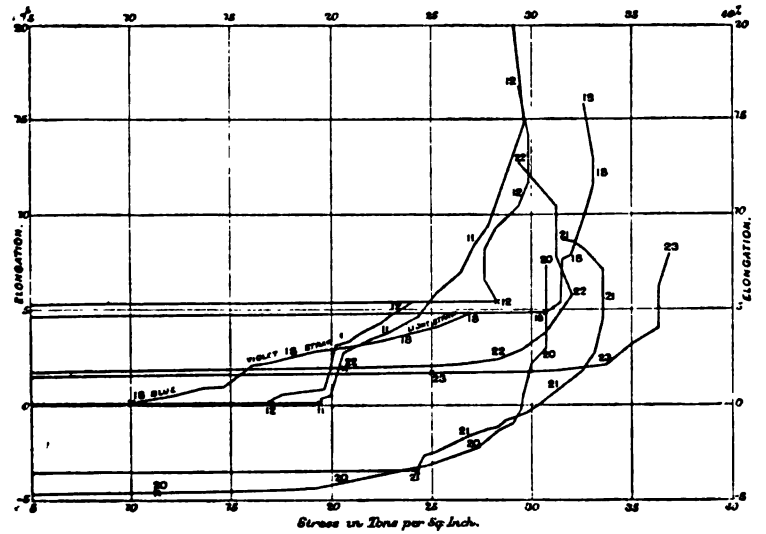


FIG. 19.



Sir FREDERICK BRAMWELL, President, said that the Author had most modestly put forward his views, but he was quite sure that the members would feel that a more valuable Paper had rarely been brought before them. The subject had been dealt with simply and clearly, both in the Paper and in the Author's

Sir Frederick Bramwell.

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Sir Frederick Bramwell.

verbal observations, and it had given rise to a discussion that he thought he had never heard surpassed in that room. The question of the effect of subjecting soft steel to the temperatures referred to, was not new to him. He wished to be allowed to refer to one instance which was, he thought, of a very convincing character. There was a time when artillery was made of cast-iron, and this was succeeded by a time when it was made of tubes reinforced by envelopes formed of wrought-iron coils welded together. The transition from that mode of constructing artillery, to the modern system of the use of steel to the exclusion of all other metal, was a gradual one, and those who had been in the habit of making envelopes by the welding of iron coils, thought that the best mode of making steel envelopes must be by the use of weldable steel and the welding it into coils. Artillery was made in that way, and he believed with very good results. But in carrying out a series of experiments while the steel, on one occasion, was under the hammer, the hammer became gagged, or there was some interference to its working, so that when the hammer was got to work again the steel had cooled down to a blue-heat. On striking it a blow at this temperature the whole coil went to powder, crumbling into pieces about the size, it was said, of salmon's roe. Further, engineers knew that one of the advantages of a snapped head, or a machine-riveted head in a boiler, as compared with the old form of rivet made by hand-hammering, was that all the work was put on the rivet while at a good heat; when, however, a rivet was made by a pair of riveters with hand-hammers, the men could not be induced to leave off hammering, they liked to hammer during the time when the rivet was cooling, although it was always believed that hammering a rivet at that stage of heat—an iron rivet even—was calculated to do harm. Steel was not the only metal that had a critical temperature. Muntz metal, for example, if forged perfectly cold, could be made into a bolt like the one produced; and if forged hot at a proper heat it would be like the second one produced—both excellent forgings; but if it were attempted to forge at an improper temperature, the work would crumble to pieces as shown by the sample. The samples were lying on the table. He had asked Mr. William Anderson to prepare them and to speak of them. He had prepared them, but had forgotten to mention them, and he had therefore brought them under the notice of the Institution. He thought also he was right in saying that zinc had a critical temperature at which alone it could be safely worked. So that, in other metals besides steel, there was a critical temperature which

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must be observed to work with safety. Dr. Pole had referred to instances in which wire had borne great tensile-strains, and had preserved that which he had called a high elastic limit, nearly up to breaking-point. Sir Frederick Bramwell confessed that he did not know what elastic limit was. There had been a discussion in that room before about that question in regard to artillery, and he had pointed out that all the complex mathematical formulas introduced into the Paper, to ascertain what was the maximum tension that might be used without interfering with the elastic limit, had started upon the supposition that the wire to begin with was a straight wire. Whereas the fact was, as was very well known, that wire, in the very act of its manufacture, was pulled through the "triplet" by which it was formed in the draw-bench, by winding the wire upon a drum, the revolution of the drum pulling it through, and thus the wire, as delivered from the hole, was instantly subjected to an amount of bending which put it into a permanently curved form, and that wire, therefore, ought to be rejected if the passing of the elastic limit was of so much importance as was supposed; because clearly when a straight wire was bent into a circle, and retained that circular form, the elastic limit must have been passed. With respect to the mode of bending specimens of steel and other metals, it had been thought by those who had drawn up the rules for the tests of steel for the construction of artillery (drawn up primarily by Mr. William Henry Barlow, Sir Frederick Abel, and himself), that it was very undesirable to continue the use of the hammer-test for bending. They never knew exactly how the steel was being treated, and therefore they determined that all bending-tests should be made by means of a semi-circular ended presser pressing the specimen through an aperture with rounded sides, doing it at a definite rate and with absolute certainty. Unless that was done they could never be quite satisfied in comparing one set of bendings with another. It was quite true that when a man passed the whole of his time, as prior to that arrangement men sometimes practically did, in hammer-testing strips of steel all of one size, he acquired an amount of skill which was very likely to produce a fair result; but if one test was made, say in Woolwich, and another in Newcastle, there might be two different men using the hammer in different ways; and it was to meet difficulties of that kind that they had thought it expedient to adopt a mechanical mode of bending.

Sir Frederick Bramwell.

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Correspondence.

Mr. Adamson. Mr. DANIEL ADAMSON observed that the Paper was wanting in this particular, namely, that it took no cognizance of what was alloyed with the iron. The only difference in all the irons in the world, arose from the alloys with which the metallic iron was combined. This much he could say, in proportion that the alloying element was increased, so was the infirmity of such metals at colour heat augmented. Blue heat certainly was not an exposition of the condition; as it was manifested in several colours, and the higher alloyed metal would break up at black heat. In a Paper which he had read at Paris, "On the Mechanical and other properties of Iron and Mild Steel,"[1] he had given in an Appendix the chemical compositions, which materially assisted in understanding the infirmity of alloyed metals. The greatest practical importance he attached to colour heat was relative to railway axles, in which the heat caused the grease to boil off. The temperature then was dangerous; and such axles subjected to concussions were liable to be broken. The fly-shafts of marine engines could only be protected in case they became overheated, by stopping the engines, and allowing the shafts to cool. Cooling them suddenly in cold water was sure to set up local cracks, and to end in complete rupture.

Mr. Andrews. Mr. T. ANDREWS observed that the interest of the Paper would have been enhanced had the Author given a complete chemical analysis of the various steels and wrought-iron employed, and also ascertained the exact temperature conditions of the metals at the time of observation in each experiment. As regarded wrought-iron, his own practical experience generally coincided with the Author's experimental observations; neither iron nor steel ought to be worked at a blue, or black heat, or considerable injury might ensue. It appeared from a Paper "On the Modification of tensile strength and ductility which Iron and Steel undergo when heated to between certain temperatures," read by Mr. E. Cornut before the seventh congress of Engineers-in-Chief of the Association of Proprietors of Steam Boilers, held at Bordeaux,[2] and from the observations of Mr. C. Huston, Mr. C. Walrand, and others therein alluded to, that there was a peculiar modification of tensile

[1] The Journal of the Iron and Steel Institute, 1878, p. 383.
[2] "Iron," May 1st, 1885.

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Mr. Andrews.

strength and ductility in iron and steel at temperatures ranging from about 572° to 662° Fahrenheit. These researches, and the Author's experiments might therefore be advantageously considered together, and reference might also be made to a Paper by Mr. W. Worby Beaumont, Assoc. M. Inst. C.E., "On Modern Steel as a Structural Material."[1] The strength of both iron and steel was undoubtedly materially influenced in a variety of ways by temperature conditions; low temperature increasing the tensile endurance to some extent, but reducing the power of resistance to sudden impact. Mr. Andrews was engaged in investigating some of these conditions, and the effects of varied temperature on the strength of large forgings, in an extensive research. Steel was a complex crystalline chemical substance, and did not appear always to manifest that complete homogeneity and uniformity of structure and behaviour which some persons considered it to possess. The elaborate research, "Théorie Cellulaire des Propriétés de l'Acier," communicated in 1885, to the French Academy of Sciences by Messrs. Osmond and Werth, of the Creusot Works afforded some valuable indications of the peculiar internal structure of steel. Other of its variable properties when under corrosive action in sea-water had been electrically considered in some of Mr. Andrews' recent Papers.[2] Mr. Osmond had communicated with him stating that the interchanges of electro-chemical positions of steels in sea-water, and the mode of their corrosion, which Mr. Andrews had observed, had tended to confirm electrically the experimental views he had expressed as to the cellular structure of steels. The microscopic researches of Dr. H. Clifton Sorby, on the structure of steel,[3] also further greatly assisted in affording information on the physical structure and nature of steels, an apprehension of which was essential to a correct prognosis of the ultimate suitability and permanency of these newer metals for structural purposes. Owing to its compound crystalline character steel might, perhaps, be regarded as more liable to injury from varying temperatures than wrought-iron, especially in circumstances where inequality of expansion and contraction might obtain. Mr. Isherwood, of the United States Navy, had observed that steel shafts were liable to become brittle under certain conditions of high temperature, which practically might arise in the case of heated

[1] Society of Engineers, Transactions for 1880, p. 109.

[2] Transactions of the Royal Society of Edinburgh, vol. xxxii., Part I., p. 205. Minutes of Proceedings Inst. C.E., vol. lxxvii., p. 323; vol. lxxxii., p. 281.

[3] The Journal of the Iron and Steel Institute, 1885, p. 203.

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Mr. Andrews. necks, or journals. He remarked "that the increase of temperature referred to increases the brittleness of iron and steel, and of steel more than iron, as it accounts for many fractures impossible to otherwise explain of marine engine shafts." Again, "steel shafts are much more subject to these accidents than iron ones, and at high temperatures the brittleness of steel due to temperature is greater than that of iron." The treacherous after behaviour of steel in some instances, such as the failure of steel steam-boilers recently recorded by Mr. Maginnis,[1] and the disastrous sudden fracture of the steel locomotive crank-axle at Penistone on the 16th of July, 1884, which had only been in work about fourteen months, even where it passed satisfactory tensile tests, etc., could not always be accounted for on the Author's supposition of temperature injury during manipulation. Moreover, steel generally was liable to develop growing internal flaws, possibly emanating from some of the unevenly distributed nuclei of its crystalline formation.[2] These growing flaws, which, owing to the homogeneity of its nature were always more or less liable to develop in steel, constituted a serious source of unknown danger considered by "L" in his letter thereon, in "The Engineer," December 25th, 1885. Such flaws seemed at present undetectable, and generally with steel no intimation was afforded previous to the occurrence of fracture, the behaviour of iron in most instances being the reverse in this respect. These circumstances showed that there was considerable risk in the use of steel for many purposes, and although steel primarily endured a higher tensile-strain than wrought-iron, it did not necessarily follow that this was an infallible indication of its universal applicability, after endurance, or permanent stability under all conditions.

Sir Henry Bessemer. Sir HENRY BESSEMER cited one special case (which came under his personal observation) in support of the views put forward in the Paper, and he agreed with the Author in believing that many of the so-called "mysterious properties of steel" owed their peculiarity to having been worked at a "half-heat." On one occasion he had seen a plate, which rolled like copper at a white heat, go to pieces in the act of rolling when it was continued after the plate had ceased to be visible in the dark; and it was found that these broken pieces could be rolled when quite cold without fracture, and were perfectly tough strong metal when annealed. This was an extreme case and was, he had no doubt, largely the result

[1] "The Engineer," 1885.

[2] The Journal of the Iron and Steel Institute, 1885, p. 272.

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of an overdose of phosphorus in the metal of which the plate was made. Sir Henry Bessemer.

Mr. J. COLVILLE, while generally approving of the Paper, must take exception to the remarks with respect to the conduct of mild steel after having successfully withstood hot bending, even if at a blue heat, as he certainly did not think such "almost sure to fly to pieces when cold;" neither could he agree as to plates suffering by being laid upon each other immediately after being rolled. Mr. Colville.

Mr. A. COOPER regarded the subject of the Paper as a most important one, both to the makers and to the users of steel. He was sure if it were better understood and appreciated by ship-builders and boiler-makers, much less would be heard of the mysterious crackings of steel-plates and bars, as from numerous instances which had come under his knowledge he believed that fully one-half of the plates and bars that cracked in the hands of the users whilst being worked, or after being worked, "hot," as they termed it, failed solely because the working had been continued after the temperature of the piece had fallen below dull red; as subsequent testing had proved in almost every instance that the steel had been not only chemically correct, but also that it would stand almost any amount of punishment either when red-hot or when quite cold, but at the medium temperature, probably when passing through the "blue heat" period, it had been unreliable. Mr. Cooper.

Mr. F. W. DICK observed that there was very little that could be taken exception to, and very little to add to the Paper. It was a matter for congratulation that the Author so strongly advocated the working of steel when red-hot, or when cold only. The Steel Company of Scotland had for many years endeavoured to impress upon the users of steel that on no account must it be worked at a blue heat. Another important point noticed by the Author, and one which should be kept well in mind, was that all steel which had been worked at a blue heat should be subsequently annealed; it should be brought to a full red heat. The manner of cooling, if not too rapidly effected, was not of so much importance. The Author had referred, page 122, to the apparent effect on "medium-hard" steel, of long continued exposure to blue heat. It would be well not to give too much credence to this until the circumstances were fully known. Probably the test-pieces were "strained," by shearing or otherwise, before being placed in the core-oven. The blue heat was insufficient to relieve such strains. It would be noticed from the last line of page 122 that the test- Mr. Dick.

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Mr. Dick. pieces had seemingly been sheared. Plates were constantly stacked hot in rolling-mills, and any brittleness induced in the manner surmised by the Author could not have escaped notice. The points to be remembered in this connection were:—

1st. Initial strains existing in steel were not eliminated by raising to a blue heat. The heating must be continued to full redness before such strains were got rid of.

2nd. Steel strained at a blue heat and allowed to cool, continued in a state of strain, and was much injured, and this injury was much greater than if the steel had been strained while cold.

3rd. Steel which had been injured by strain at a blue heat was restored to its original condition by raising it to a red heat and allowing it to cool.

Some six years ago experiments were made at the works of the Steel Company of Scotland to determine the influence of manganese on the behaviour of steel at the blue heat. Wide variations in the quantities of manganese present (from 1 per cent. to 0·2 per cent.), produced no apparent differences in the results, and this led to various brands of iron plates being subjected to similar tests. It was needless to add that the iron plates were similarly affected at the blue heat. The presumption was that manganese was not the cause, although it might have some effect upon the brittleness of steel at this temperature.

Mr. Gillott. Mr. THOMAS GILLOTT observed that the tenderness of iron at a heat below redness had been known in the best Yorkshire iron-works for probably more than fifty years, and "blue shortness" was a recognized weakness. So far as his experience went, steel was more troublesome in this respect than iron, but it was scarcely correct to attribute failures generally to working either iron or steel after a red heat had been passed. Nor need many of the cracks often heard of be called mysterious, if the history of failing plates was fully traced from the ingot onwards. Contraction cracks in steel ingots were often caused by the irregularities of a worn-out ingot mould; and some cracks could be welded up when hammering, but would always leave a line of weakness across the finished material. A slight red shortness might cause a flaw when being worked hot that would prove a weak spot when cold, in a plate that would, when tested in the usual way, prove as ductile as could be wished. One condemned by him after rolling had slight cracks across in one part of a plate 14 feet by 6 feet 10 inches by $\frac{5}{8}$ inch, and although so ductile that it was scarcely possible to break shearings after heating and quenching, there was a line of weakness across the plate

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due to red-shortness that doubtless would have fractured in work had it been used. An analysis of the red-short part gave carbon 0·120, silicon trace, sulphur 0·041, phosphorus 0·068, manganese trace. The chief trouble generally arose with large plates, such as those used for the fronts of marine-boiler furnaces, and requiring to be flanged for the boiler-shell and furnaces. Could these plates be flanged in a press at one heat, no doubt less would be heard of failures in steel; but taking the results of his own experience, in a total number of about two thousand five hundred furnace front plates for marine-boilers, there were nearly nine hundred different patterns; so that, for commercial reasons, flanging in successive heats, and straightening at a single heat afterwards, could scarcely be avoided. Could marine-engineers agree on standard patterns, better methods of flanging than those generally in use might be adopted. After straightening and facing the flanges, slight adjustments of the flanges to the extent of not more than ¼ inch had to be made, because the time during which plates would remain hot enough for working, when not over-heated, is not more than five to ten minutes, depending on their thickness. These adjustments were generally effected by applying heaters and setting by hand-hammers, or, in other words, performing the work in the precise way objected to by the Author. The heaters were not applied "to take the chill out," as they were just as much used in summer as in winter, but to enable the work to be done; and as many flanges for attachment to the shell of a marine-boiler were as much as 9 inches deep for a plate ⅞ inch thick, some method of adjusting had to be adopted which would locally heat a plate for a slight adjustment, without causing inaccuracies in parts already correct. Any annealing after such an operation would simply entail a further adjustment, as it was hardly possible to draw a large flanged plate out of a furnace without distortion, and it rarely happened that such plates were flat when allowed to cool freely.

Mr. Gillott.

There was little doubt that more plates were adjusted by applying heaters, instead of again using the furnace, than was absolutely necessary; but it was open to question whether two or three furnace-heats did not damage iron or steel more than applying heaters after one furnace-heat. No accurate record was kept of fractures in Yorkshire iron when adjusted as described, but he thought that not one plate in twenty would be cracked by setting or riveting, and such cracks, when there were any, would nearly always occur where parts had been somewhat over-heated in the furnace. The difficulty of obtaining perfectly uniform heat in a

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Mr. Gillott. furnace 16 to 18 feet long and 8 to 10 feet wide, with a large semi-circular plate having some flanges 9 inches and others 5 inches deep, occupying the available length and breadth of the surface, would be recognized by those having experience in such work; so that it was by no means easy to secure sufficient heat all over it without a slight excess in some part. Work of this kind could not be treated like the small strips used by the Author, and his own impression was that iron was less damaged by blue heats for setting than it would be by additional furnace-heats.

As regarded mild steel, his experience led to the conclusion that it was more liable to fail at a blue heat than best iron. Frequently, when through the drawing-in of the unflanged edge of a furnace front plate while being flanged, a furnace-hole had been oval to the extent of $\frac{3}{4}$ inch, and a stretcher-bar had been put across the shorter diameter, whilst the plate was hot for straightening, so as to obtain greater roundness when cold, by the short diameter contracting on the stretcher, and without injury in the case of iron, but not so with steel. As an instance of this, a mild steel plate for the furnace front of a boiler, 12 feet 3 inches in diameter, with two furnace openings of 3 feet 6 inches, had a stretcher-bar placed across the vertical diameter of one furnace that was slightly oval, and it cracked the flange through at the upper point, although the distance between the upper unflanged edge and the inside of the furnace opening was only $5\frac{1}{2}$ inches. Yet this plate, which was full $\frac{11}{16}$ inch thick, when tested showed a tensile strength of 28·8 tons per square inch, and elongated 25·625 per cent. in 10 inches, the contraction of area being 42 per cent., and the fracture silky. The strips when heated and quenched were bent to $1\frac{1}{2}$ inch between the folds, and the carbon sampled 0·15 per cent.

With respect to the composition of steel, his own experience appeared to indicate that if mild steel contained from 0·10 to 0·15 of phosphorus, although capable of sustaining severe bending-tests as it left the rolls, it tended to become brittle by successive heats, far more so than steel in which phosphorus was low. The radius of the curves to which the specimens were bent was also of great importance, but was probably kept uniform, although this was not easy.

Mr. Hudson. Mr. J. C. HUDSON hoped to be able to indicate a direction in which experiments might be made to find the reason why steel was difficult to work at a blue heat, and more so than when hot or cold. He would consider briefly plate-steel, such as was used in boiler-making, bent when cold, when red-hot, and when at a blue heat. In bending a piece of steel-plate from straight to a curve,

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the outer surface of the curved plate would be stretched, and the inner surface compressed. But as the power of steel when cold to resist a tensile strain was usually considered less than that required to compress it, it might be supposed the plate would be distorted by stretching more than by compression, in order to accommodate itself to the altered condition of a plate when straight and when bent; and as steel had a great range of elongation under a tensile strain before breaking, evidently a plate might be bent a number of times before fracture. The elasticity of steel would also aid this plate in resisting fracture. Steel when red-hot, or at a higher temperature, would stand bending, as the metal then was more plastic, and the power to resist compression was considerably reduced, and when bending it the outer surface of the curve might be stretched, but the inner surface would be compressed, which compression would reduce the stretching on the outer surface, and considerably reduce the fatigue on the metal. But suppose that steel at a blue heat had lost the power of elongation, or had it considerably reduced, and that the power to resist compression remained practically the same as when cold, then it would appear that a plate bent under this condition of temperature would be rapidly fatigued on the outer surface of the curve, and a fracture would follow owing to the metal being then unable to resist the strains consequent to cooling. From the foregoing he ventured to suggest that the reason why steel was difficult to work at a blue heat could be readily investigated by experiments on the metal at this temperature, and under tensile strain, when he believed it would be found to have lost to a great extent its power of elongation, when also its power to resist compression was not much reduced. His observations of the fracture of steel worked at this temperature confirmed him in this view. Mr. Hudson.

Professor D. E. HUGHES observed that if blue heat did affect the strength of steel, then it could only be through some molecular change in its structure, and this could be easily determined by testing its magnetic capacity. The day must soon come when a sample of iron or steel would no longer be broken or destroyed in order to find out the molecular condition of its structure. Electricity or magnetism should be able to do this. He had already shown, in his induction balance, that by its aid the slightest change could be detected in the molecular structure of iron and steel, and any strain or flaw be at once found out, and he had shown that, when a current of electricity was sent through a wire or bar of iron, a very slight mechanical strain reduced the self-induction of the bar 40 per cent. It was by physical means alone that the Professor Hughes.

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Professor Hughes. hope could be indulged of penetrating inside a bar of iron or steel, and the exact molecular structure be revealed at any given point or after any given treatment; and although at present no practical instrument existed suitable for tests on all shapes and sizes of iron or steel, the day must come when the present almost brutal method of testing iron and steel would give way to more scientific, and far more reliable tests than could be obtained without the aid of physical means. He had tried to solve this question—he was trying still—and others would succeed, even if he failed, as the problem had been already solved from a scientific point of view. Needing only a practical instrument, which could do in rough usage and on any scale, that which could at present be accomplished with comparatively small samples.

Mr. Sandberg. Mr. C. P. SANDBERG remarked that the alleged great advantage of the use of steel instead of iron, in effecting a saving of 25 per cent. of fuel in steel boilers, through the possibility of using higher pressure, was enough to prove its progress, but it would necessarily be checked by this unexplainable phenomena of cracks, snapping, and fractures, as stated in the Paper. It had been the same on the introduction of steel rails, but this has now almost disappeared, and Bessemer steel for rails was now produced with great regularity. This, and the absence of fractures, had been principally arrived at by reducing the hardness, though at the expense, unfortunately, to some degree in their wearing qualities; and he anticipated that if a demand should now arise for harder steel rails, there would be some increase of fractures. It would be of great importance to treat this hard steel more carefully than mild steel, by finishing the working or rolling it out before it had declined to a blue heat, and then cooling it slowly and regularly without cutting it when cold. In fact, it should always be borne in mind that the less steel was touched after leaving the rolls, the less was the probability of spoiling it, and this applied especially to hard steel. Mr. Sandberg strongly advocated the employment of chemical, as well as mechanical tests in the manufacture of steel rails, and that the latter should be subjected to the same conditions of trial as they would be exposed to in actual practice. He much regretted that there was a total absence of chemical tests to represent quantitively what was meant by mild, very mild, and hard steel. The Author should have given, in conjunction with the mechanical tests, the constituents of the various samples of steel, including not only the carbon and silicon, but the other constituents, such as phosphorus, sulphur, and manganese. If makers and inspectors could afford to do this for steel

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rails, they could surely do so for steel plates. He was not prepared to say that the chemical results would in all cases explain abnormal or curious features developed by mechanical testing, but they would in most instances explain matters, and thus give the key to correction. The chemical testing of different productions, obtained by different processes, was of great value as a guide; but serviceable steel might be had of variable composition, therefore he did not approve of specifying for steel of one fixed chemical composition only. He should like to learn from the Author how the temperatures defined as blue, and black heat had been taken. Mr. Saudberg.

Where bad results, such as snapping and fractures, occurred in steel, it was no easy matter to decide whether such behaviour was due to bad steel or to bad workmanship; for improper treatment could spoil even the best steel; and it was in this view that the Paper was of great practical interest to all users of steel-plates. However, the results of his inspection of steel-rails, and which might also apply to steel-plates, tended to show that regularity of production was rarely if ever obtained from a new maker at the first start, from want of experience both of the machinery and of the mill, which was not got in a day; and this might explain occasional bad results; but it should by no means be disheartening, for it would soon disappear, and was almost unavoidable. As regarded a statement that inferior German steel had been used for bayonets supplied to the British army, he thought the saving in first cost of an article like that was wrong. Such articles should be made solely of Swedish steel, one of the peculiar advantages of which was a high degree of "body," or a capacity to stand many repeated heatings without becoming soft.

Mr. H. SHARP stated that the injurious effects of working steel at a blue heat had been well known to many steel-makers for some years. An elaborate series of experiments on this subject had been made by Mr. J. F. Barnaby, Admiralty Overseer, at the Cyclops Works, Sheffield, the results of which had been published by the Admiralty,[1] and he felt that he could not add more to the subject than had been published by that gentleman. There was one point, however, which he thought it might be well to mention, namely, the practice which was carried on in many boiler-yards of making steel plates hot preparatory to bending them in plate-bending rolls. A long experience has satisfied him that this Mr. Sharp.

[1] "Influence of temperature on the strength and ductility of steel and iron." 1881. "Effects of heat on the bending qualities of iron." 1881. "Effect of repeated heating and cooling on tensile strength of steel and iron." 1882.—[Inst. C.E., Tracts folio, vols. 29, 32.]

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Mr. Sharp. process was most injurious to steel, and he had no doubt that many of the unsatisfactory results given by steel boilers might be attributed to this cause.

Mr. Webb. Mr. F. W. WEBB submitted the following particulars of bending-tests and tensile-tests made in the testing departments of the Crewe Works of the London and North-Western Railway :—

I. BENDING TESTS on STRIPS SHEARED from ONE ORDINARY ANNEALED BOILER-PLATE (62-12 V). Carbon 0·19. Bent through angle of 135°, forming where bent angle of 45°.

Number of Pieces.	Number of Times Bent and Straightened.	Remarks.
2	2	Two strips sheared off plate (not re-annealed). Broke during second straightening.
3	7¾	Three pieces made red-hot and tested in that condition. Broke during eighth straightening.
3	1	Annealed and made blue-hot and bent in that condition. Broke during first straightening.
1	Nil	One piece hammered at a blue heat. Broke during attempt to bend at a blue heat.
2	Nil	Hammered at a blue heat and allowed to get cold. Broke during attempt to bend.
2	2	Hammered at a blue heat and then annealed; bent cold. Broke during second straightening.
2	3	Pieces of the ordinary plate simply annealed and bent cold. Broke during third straightening.

II. EXPERIMENTS on the TENSILE-STRENGTH of BOILER-PLATES TESTED UNDER THE CONDITIONS STATED BELOW.

Number of Pieces Tested.	Breaking Weight per Square Inch.	Extension taken on a Length of 10 inches.	Remarks.
	Tons.	Per cent.	
3	30·96	23·6	Ordinary plate. Annealed and tested cold.
1	38·04	4·3	Bent three times while blue-hot to angle of 30°, and tested cold.
2	35·24	10·05	Hammered while blue-hot. Tested when cold.
2	30·26	23·8	Hammered while blue-hot, and annealed. Tested cold.
3	31·95	7·5	Bent once when blue-hot. Tested cold.
3	30·74	22·1	Bent once blue-hot. Annealed, and tested cold.
3	31·65	22·5	Bent red-hot. Tested cold.
3	32·42	12·5	Annealed plates. Bent, and tested cold.
3	30·60	23·63	Annealed plates. Bent, re-annealed, and tested cold.
2	38·80	11·4	Tested while blue-hot.

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Mr. Webb.

From the bending-tests, it was obvious that plates which had been hammered, bent, or rolled while blue-hot were very liable to fracture when afterwards subjected to bending or concussive action. The annealing appeared to restore the ductility to a plate which had been worked blue-hot, to a great extent, but not completely. The bending was performed by gentle blows from a steam-hammer; and the angle 135° was selected merely for convenience. None of the plates under tensile strain broke across the place where they were bent. The main effect appeared to be on the elongation of the plates, which was much reduced by hammering and bending either when blue-hot, or when cold; though in each case it was nearly restored by annealing. The increased tensile-strength of the plates tested blue-hot was remarkable. The plates also stretched by fits and starts. The elongation was less, and the strength greater than ordinary plates. All the tests, both bending and tensile, were from the same boiler-plate (62–12 V).

*** The results of a series of experiments, undertaken by Mr. W. Parker, M. Inst. C.E., at the suggestion of a Member of Council, on the question of steel becoming brittle when worked at a blue heat, will appear in a future volume.—Sec. Inst. C.E.

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2 February, 1886.

SIR FREDERICK J. BRAMWELL, F.R.S., President, in the Chair.

The following Associate Member has been transferred to the class of

Member.

MATTHEW CURRY.

The following Candidates have been admitted as

Students.

HERBERT ASHLEY.
ALFRED BROOKS.
BERTRAM CHATTERTON.
HUGH DONALD CLARK.
SIDNEY WARD CONYERS.
FRANK WILLIAM DODD, Wh.Sc.
FREDERICK HOPE FISHER.
HARRY LUTTRELL GODDEN.
ANDRÉ PIERRE GRIFFITHS, Wh.Sc.
THOMAS CHARLES JOHN HARRISON.
WALTER HERBERT HUTTON.
WALTER COKE KERR.
HUGH HANNAY MCCLURE.
LOUIS MARTINEAU.
CHARLES MILDRED.
PHILIP FRANCIS ODDIE.
VALENTINE REGINALD PRINCE.
RICHARD HUGHES ROBERTS.
DAVID HENRY SMALL, Jun.
JAMES CRAVEN MAURICE STANTON.
ALFRED DEARMAN STEVENSON.
FRANCIS VILLIERS TAYLER.
ARTHUR HERBERT WALLER.
JAMES RONALD WATSON.
LEONARD LATHAM WICKHAM.

The following Candidates were balloted for and duly elected as

Members.

JOHN FIELDING.
WILLIAM GOOD.
WILLIAM MATHER, M.P.
EDGEWORTH RICHARD USSHER.

Associate Members.

GUSTAVE VALENTINE ALSING.
CYRIL TALWORTH BATCHELOR.
ARTHUR EDWARD BROWN.
CORNELIUS EDWARD CARDEW.
WELLINGTON CARRINGTON.
FREDERICK HENRY CHEESEWRIGHT.
WILLIAM COATES.
JAMES COLLIE, Stud. Inst. C.E.
WILLIAM CROSS.
JOHN EDWARD EVERETT.
BENJAMIN KINGTON FINNIMORE.
JOHN WILLIAM FLETCHER.
CHARLES HEYLAND FOX, Stud. Inst. C.E.
WILLIAM JEREMIAH HALL, B.C.E.
WILLIAM JAMES HEDGMAN, Stud. Inst. C.E.
ALEXANDER JABEZ HOGG, Stud. Inst. C.E.
VINCENT ALEXANDER LAWSON.

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HAROLD EDWARD LOCKHART, Stud. Inst. C.E.
THOMAS BELL LOWE.
HENRY MASTERTON.
THOMAS MITCHELL.
JAMES MONCUR.
ERNEST FREDERICK MORANT.
THOMAS NORTH MULLIGAN, A.K.C., Stud. Inst. C.E.
JOHN SAMUEL PAINE.
ALFRED LINDLEY PEACE.
FRANK EDWARD PRIEST, Stud. Inst. C.E.
HERMAN ALFRED ROECHLING.
JOHN HOWARD SHAW.
JOHN DANN SMELT, Stud. Inst. C.E.
WILLIAM STERLING, Jun., Stud. Inst. C.E.
PETER CROSLAND TEMPEST, Stud. Inst. C.E.
GEORGE WILLIAM THOMPSON.
PETER WILLIAMSON THOMSON.
GEORGE PEACOCK WEIGHT.
GILBERT PERCY WILLIAMS, Stud. Inst. C.E.
EDMUND RICHARD WINDOW, Stud. Inst. C.E.
WILSON WORSDELL.

Associates.

WILLIAM HOPE.
CARMICHAEL LIGHT YOUNG, *Capt.* R.E.

The discussion upon the Paper on "The Injurious Effect of a Blue Heat on Steel and Iron," by Mr. C. E. Stromeyer, occupied the whole evening.

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9 February, 1886.

Sir FREDERICK J. BRAMWELL, F.R.S., President, in the Chair.

The discussion upon the Paper on "The Injurious Effect of a Blue Heat on Steel and Iron," by Mr. C. E. Stromeyer, occupied the whole evening.

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16 February, 1886.

Sir FREDERICK J. BRAMWELL, F.R.S., President, in the Chair.

(*Paper No. 2122.*)

"The River Seine."

By LEVESON FRANCIS VERNON-HARCOURT, M.A., M. Inst. C.E.

THE Seine is the most important river of France, both on account of its being the highway for a flourishing inland trade, and in consequence of the extensive engineering works which have been carried out for its improvement. Its basin has also formed the subject of very careful hydrological investigations, for it was on the Seine that the systematic prediction of floods was first established, which has since been adopted for other French rivers. Like the Thames, it flows past the capital of the country, to which fact it owes a portion of its importance, though the centres of maritime trade to which it gives access are situated lower down; like the Thames, also, it has been made the receptacle for the sewage of the largest city on its banks, though recently it has been relieved of a portion of this nuisance by the establishment of extensive irrigation works on the plains of Gennevilliers (Plate 2, Fig. 1). Unlike the Thames, however, its estuary, being less favoured by nature, has afforded scope for important works of tidal-river improvement; and the distance of Paris from the sea has led to a more thorough extension of its inland navigation.

Though references to the Seine, by the Author and others, will be found in several volumes of the "Minutes," yet neither in the Proceedings of the Institution, nor, as far as the Author is aware, in France, has a comprehensive Paper on the River Seine as a whole been hitherto published. The Author thinks that such a Paper, in a condensed form suitable for the "Minutes," cannot fail to possess much interest for engineers, owing to the great advantages afforded by giving timely warning of floods, and adopting measures for their mitigation, the large, varied, and novel works which have been executed for the improvement of the river, and the important extensions still in progress or contemplated.

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The Author proposes to investigate the subject under three distinct aspects, namely: I. Hydrology of the Seine basin; II. Inland navigation improvement-works; and III., Estuary-works. The able reports of the late Mr. Belgrand, on the prediction of the floods of the Seine, drew the Author's attention to that branch of the subject more than ten years ago. He has also had occasion, for some time past, to study the Seine under its other two aspects; having visited the Seine several times for this special purpose, and availed himself of the opportunity of discussing the question with the French engineers in charge of different sections of the river; and having brought the subject under the notice of the Institution, in a brief and somewhat fragmentary form, in portions of his Papers on "Fixed and Movable Weirs," [1] and "Harbours and Estuaries on Sandy Coasts." [2] By invitation from the Council of the French Association for the Advancement of Science, he attended their Congress at Rouen in August 1883, when the extension of the Seine training-works, and the conversion of Paris into a seaport, were the subjects to which the members of the engineering section of the Association specially devoted their attention, in the discussion of which he had the honour to take part.[3] It was also his privilege to inspect the estuary-works, in April 1883, in company with the late Mr. Lavoinne, then Engineer-in-Chief of the tidal Seine, who, in a memoir [4] published since his death, has left his views on the extension of the training-walls as a valuable legacy to his successors.

I. HYDROLOGY OF THE SEINE BASIN.

The area of the Seine basin is 30,370 square miles, of which 3,750 consist of sloping impermeable strata, 3,760 of flat impermeable strata, and 22,860 square miles of permeable strata; so that the permeable strata occupy about three-fourths of the basin. The flat impermeable strata, moreover, have little influence in the production of torrential floods, and therefore practically only about one-eighth of the basin is of a strictly torrential character (Plate 1, Fig. 1). Accordingly the Seine possesses the characteristics of a gently-flowing river, differing in this respect from the other principal rivers of France, the Rhone, the Loire,

[1] Minutes of Proceedings Inst. C.E. vol. lx. p. 30. [2] *Ibid.*, vol. lxx. p. 20.
[3] Association Française pour l'Avancement des Sciences, 12e Session, Rouen, 1883, p. 293.
[4] "La Seine Maritime et son Estuaire." E. Lavoinne. 1885.

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and the Garonne, whose basins consist mainly of impermeable strata.

The Seine itself has been divided into four natural sections, with distinctive appellations; namely, the Little Seine, from its source to its confluence with the Yonne at Montereau; the Upper Seine, from Montereau to Paris, just above which place one of its most important tributaries, the Marne, flows in; the Lower Seine from Paris to St. Aubin, near Elbeuf, at the limit of the tidal flow; and the Tidal Seine, from St. Aubin to the sea (Plate 2, Figs. 1, 2, and 3.) The principal tributaries which join the main river above Paris are, the Yonne, having a basin of 4,300 square miles, larger than the area of 3,958 square miles drained by the Little Seine, which it joins at Montereau; the Loing, which flows in 8¾ miles below Montereau, whose basin is 1,786 square miles; and the Marne, which drains an area of 5,290 square miles. The Aisne, with a basin of 3,282 square miles, unites with the Oise at Compiègne; and the Oise, whose basin has an area of 3,359 square miles, discharges the flow of the combined rivers into the Lower Seine, a little above Poissy. The Eure, having a basin of 2,216 square miles, falls into the Lower Seine at Pont de l'Arche, and is the lowest tributary of any note which flows into the main river; for the Rille, draining a comparatively small area, discharges into the estuary just below the ends of the training-walls (Plate 1, Fig. 1).

Flood Characteristics of the Tributaries.

Since the character of the floods of a river depends mainly upon the permeability or impermeability of the strata forming its basin, each valley has to be separately considered, in order to ascertain the nature of the flow of each tributary, and the influence of the tributaries on the main river (Plate 1, Fig. 1). None of the principal tributaries of the Seine flow exclusively over impermeable strata; but the upper part of the basin of the Yonne, being both impermeable and steep, imparts a torrential character to the river (Plate 1, Figs. 4 and 10), though not quite one-half its whole basin consists of impermeable strata (Plate 1, Fig. 1). Nearly one-half of the Marne basin is composed of impermeable strata (2,361 square miles out of 5,290 square miles); and the Marne, in the upper portion of its course, has a very marked torrential flow (Plate 1, Fig. 12). This torrential character of the river is somewhat lost in its flow through the central permeable strata, and is not fully regained in the im-

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permeable strata forming the lower basin, owing to the flatness of this part of the valley, though reinforced by the influx of some torrential streams, and notably the Grand Morin (Plate 1, Fig. 6). The Loing has the most impermeable basin of all the main tributaries, for four-fifths of its basin consist of impermeable strata. The flatness, however, of these strata, combined with a small average rainfall, causes the floods of the Loing to have comparatively little influence on the main river. The basin of the Little Seine and the Aube is traversed, about half-way up, by a band of impermeable strata; but as these strata occupy only about one-sixth of the area of the basin, and the other strata are permeable, the flow of the Little Seine is gentle before its confluence with the Yonne (Plate 1, Figs. 3 and 9). Of the main tributaries flowing into the Lower Seine, the Aisne has a wholly permeable basin, and the Oise and the Eure drain very small portions of impermeable strata. In fact, of the whole area of about 12,830 square miles draining into the Lower Seine, only 2,720 square miles consist of impermeable strata, of which six-sevenths are flat. Accordingly, the Yonne and the Marne are the only affluents which tend to impart a torrential element to the Seine floods, and give these floods a mixed character at Montereau and Paris (Plate 1, Figs. 5, 7, 11, and 13) just below the confluence of these rivers.

The influence of the nature of the strata on the floods of a river is clearly seen by comparing the flood diagrams, from November to January 1882–3, of the Little Seine at Bray, and the Aisne at Pontavert (Plate 1, Figs. 9 and 14), with the Yonne at Sens and the Marne at St. Dizier (Plate 1, Figs. 10 and 12). The different appearance of the floods of the Seine at Bray and at Montereau (Plate 1, Figs. 3, 5, 9 and 11), indicates the disturbance produced by its junction with the Yonne (Plate 1, Figs. 4 and 10); and this disturbance is still further accentuated at Paris (Plate 1, Figs. 7 and 13) by the influx of the torrential Marne (Plate 1, Figs. 6 and 12). On the contrary, the gently-flowing Aisne (Plate 1, Figs. 8 and 14) and Oise have a regulating influence on the floods of the Lower Seine, as experienced at Mantes (Plate 1, Fig. 15). The Eure has little effect on the Seine floods, for, owing to its permeable basin and the small average rainfall, its flood-rise is small except after a thaw.

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Rainfall of the Seine Basin.

The Seine basin is subject to similar meteorological influences, so that the special character of any particular season, whether wet or dry, is experienced throughout the whole basin, though the actual amount of rainfall varies with the locality.

The average amount of rainfall over the whole basin, in the twenty-three years from 1861 to 1883, amounted to 27·24 inches. In 1870 it fell as low as 19·37 inches, whilst in 1866 it reached 38·03 inches.

The rainfall at different places in the Seine basin depends primarily on their altitude, and on their proximity to the sea. Thus the largest average rainfall of 79·57 inches occurs at Haut-Folin in the Morvan district, the highest point of the basin, 2,960 feet above the sea-level, and the smallest of 19·84 inches at Paris, in the central part of the basin, at an altitude of 161 feet; whilst the rainfall increases again on approaching the sea, reaching 30·12 inches at Rouen with an altitude of only 20 feet above the sea, and 38·62 inches at Yvetot, having an elevation of 485 feet. The general distribution of the rainfall over the basin is indicated in Plate 1, Fig. 2. A comparatively dry zone, where the average annual rainfall does not attain 24 inches, extends right across the basin, with Paris in the centre, and including Mantes, Courville, Melun, and Compiègne; and a portion also of the Gatinais district is similarly circumstanced. A great part of the rest of the basin, both to the east and west, has an average rainfall ranging between 24 and 32 inches; whilst only near the sea-coast to the the west of Rouen, and near to the north-eastern and south-eastern limits of the basin, are higher averages experienced. Only small portions of the Soissonnais, La Montagne, and Morvan districts reach an average of more than 40 inches; whilst the highest part of the Morvan district, at the extreme limit of the basin, alone exceeds 48 inches.

For the purposes of hydrological investigations, Mr. Belgrand found it advantageous to divide the year into two equal portions; namely, the hot season, extending from the 1st May to the end of October, and the cold season comprising the other six months. The annual diagrams of the water-levels of the Seine and its tributaries are drawn up in accordance with these divisions, commencing with May of one year and ending with April of the following year (Plate 1, Fig. 15); but the rainfall observations still follow the civil year, so that the years of the two returns do not coincide.

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Nevertheless an analysis has been made of the rainfall observations at some of the stations, making them correspond with Mr. Belgrand's divisions. Out of the fourteen typical cases selected, only the two wettest, namely, Settons, in the high part of the Morvan district, and Yvetot, have on the average a greater rainfall in the cold season than in the hot, amounting to 54 per cent. in the winter at Settons, and 51 per cent. at Yvetot. At Bar-le-duc, the next most rainy station on the list, the average amounts of rainfall in the hot and cold seasons are exactly equal; but at all the other stations, the summer rainfall exceeds that of the winter, reaching a maximum of 60 per cent. at Paris, or half as much again as that of the winter rainfall, the difference between the rainfall of the two seasons being the greatest where the total rainfall is the least. The calculations of the averages for the two seasons have not been carried back beyond 1873; but since that year, up to 1884, the extremes of the average rainfall throughout the basin have been 6·10 inches (1873–74) and 17·24 inches (1876–77) in the cold season, and 11·61 inches (1874) and 19·64 inches (1878) in the hot season. These returns indicate that the cold season is quite as much subject to periods of dryness as the hot season; whilst in the few years for which statistics are given, the range of variation is greater in the cold season. The distribution, however, of the rainfall throughout the basin is more uniform in the winter months, both in average and extreme years, than in the summer owing to the local predominance of summer storms.

Floods of the Seine.

Although the rainfall over the Seine basin is greater in the summer than in the winter months, yet, owing to the evaporation and the dryness of the soil in the hot season, the rains of summer have comparatively little influence on the flow of the river. Any rains falling between the middle of May and the middle of October produce little effect on the river, except under very unusual conditions; whereas any heavy rain during the winter months, falling upon a saturated soil when evaporation is inactive, causes a flood, of which the height depends upon the amount of saturation of the basin by previous rains and the duration of the fall. Accordingly the occurrence of high floods is due to the concentration of the rainfall at special periods of the year, and not to a high average rainfall throughout the year. These fundamental principles are amply verified by the diagrams (Plate 1, Fig. 15) representing the daily height of the Seine at Mantes from May 1st, 1872, to April

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30th, 1884. All the floods of the Seine during those twelve years occurred during the cold season; whilst the rainiest year, 1878–79, with a rainfall of 35·67 inches, did not experience floods of unusual height, four out of the twelve years showing higher floods; and the highest flood, in March 1876, occurred in a year whose rainfall was only 32·36 inches. The wettest hot season, moreover, of 1878, with a rainfall of 19·64 inches, occasioned less rise in the river in mid-summer than in July-August 1879 with a half-year's rainfall of 15·75 inches. The most rainy of the cold seasons also, (1876–77), with a rainfall of 17·24 inches, did not coincide with a period of greatest floods; for the cold seasons of 1875–76, 1878–79, 1879–80, 1880–81, and 1882–1883 experienced higher floods, with rainfalls respectively of 14·64, 16·02, 9·05, 14·56, and 17·01 inches. The flood of January, 1880, was somewhat anomalous, being caused by the breaking-up of the ice, for it rose with unusual rapidity and was of very short duration, and occurred in an exceptionally dry winter season; whereas in the other two cold seasons of minimum rainfall, 1873–74 and 1881–82, with rainfalls of 6·10 and 8·58 inches respectively, there was an absence of floods. Nevertheless this flood forcibly illustrates the remarkable difference between the influence of summer and winter rains on the flow of the river; for whereas a rainfall of only 9·05 inches, in the winter of 1879–80, was accompanied by a rise of 23·92 feet above the zero of the Mantes gauge, summer rainfalls of 19·64 and 15·75 inches, in 1878 and 1879, only produced rises of 10·07 and 10·03 feet on the same gauge (Plate 1, Fig. 15).

The flow of a river depends upon the proportion of the rainfall which finds its way into the river; and this proportion varies with the nature of the strata of the basin, and the season of the year. The height of a river is influenced by the period occupied by the rainfall in reaching the river, and the time at which the floods of the various tributaries pass down.

The average amount of rainfall over the Seine basin above Paris has been estimated at 20,895 million cubic yards in the summer months, and 15,671 million cubic yards in the winter; whereas the average summer discharge of the Seine at Paris is only 3,605 million cubic yards, or 17 per cent. of the rainfall, and the average winter discharge amounts to 6,696 million cubic yards, or 43 per cent. of the rainfall. Accordingly, although the average summer rainfall, of this portion of the basin, exceeds the winter rainfall by one-third, the average winter discharge of the river at Paris is nearly double the summer discharge, since the percentage of rainfall which reaches the river in

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winter, is nearly two and a half times the summer percentage. This percentage of available rainfall, or coefficient of efflux (*coefficient d'écoulement*) as it has been termed, varies at different periods of the same season according to the state of saturation of the soil, and is different for the various strata. Thus the discharge of the Seine at Paris during the great flood of March 1876 gave a coefficient of efflux of 0·45; and the first large flood in the winter of 1882–83 gave a coefficient of 0·31; whilst the second flood gave a coefficient of 0·51, owing to the saturation of the ground by the preceding flood. Again, the coefficient of efflux from the Granite of the Morvan district averages 0·75 throughout the year, falling to 0·44 in the hot season, and attaining 0·98 in the winter. The Lias of Auxois affords an average coefficient of 0·49, being 0·26 in summer and 0·80 in winter; and the impermeable strata of the Upper Oise basin give a mean coefficient of 0·67. The coefficient of efflux from the sands of Fontainebleau is considered to be practically nothing, and it has been estimated at only 0·14 for the permeable tertiary strata of the Eure basin. The rainfall, however, absorbed by these very permeable strata, feeds springs, which replenish the river at a lower part of the basin.

The rain flows off sloping impermeable strata more rapidly than from permeable strata, as well as in a larger proportion. Accordingly impermeable basins produce torrential floods, which appear soon after the rain has fallen, rise rapidly, and pass away also rapidly. The floods from permeable basins rise more slowly, and are much less high, but they continue for a longer period, as may be seen by comparing the Seine at Bray (Plate 1, Figs. 3 and 9) with the Yonne at Sens, and the Marne at St. Dizier (Plate 1, Figs. 4, 10, and 12). The floods of the Seine present a mixed character, owing to the torrential nature of its tributaries the Yonne and the Marne, which, with their high floods, exercise a predominating influence on the floods at Paris. As the torrential floods from the furthest parts of the basin take only three or four days in reaching Paris, whereas the floods of the tranquil affluents only arrive four or five days later, a great flood at Paris is rarely the result of a single fall of rain. Generally a high flood is caused by the arrival of a second flood from the torrential tributaries before the long-continued floods of the gently-flowing tributaries have had time to subside. This effect may be clearly traced in the most recent high floods, namely those of March 1876 (Plate 1, Figs. 3–7), and December–January 1882–83 (Plate 1, Figs. 9–13). All the highest floods, indeed, are produced by this combination of floods of different origin. Only six instances have been re-

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corded, since 1732, of floods, due to a single fall of rain, producing a rise at Paris exceeding 13 feet; three of these were consequent on a sudden break-up of the ice, and the other three resulted from a torrential rainfall falling on a saturated soil. The anomalous character of the latest of these floods, in January 1880 (Plate 1, Fig. 15), produced by the breaking-up of the ice in the river, accompanied by a moderate rainfall, has been already alluded to; it rose 15 feet 5 inches in four days, reaching 17·71 feet on the Tournelle Bridge gauge. The preceding instance of a high flood, due to a single rise of the tributaries, occurred at the close of the hot season of 1866, and resulted from a heavy rainfall on a soil completely saturated by almost continuous rains since the middle of July; the river rose 15 feet 1 inch in six days, attaining 17·06 feet on the Tournelle gauge. The rise of the 1879–80 flood is the largest on record at Paris of these peculiar floods; but more rapid rises occurred from the breaking-up of the ice on the 30th–31st of January, 1795, and the 25th–26th of January, 1830, when within a period of two days the river rose 13¾ and 14¼ feet respectively. The greatest actual height of these floods was attained by the flood of February 1784, marking 20·17 feet on the Tournelle gauge; and the flood of January 1830 comes next, with a height of 18·70 feet.

As the flow of the Seine is of a mixed character, in which the gently-flowing streams predominate, very high floods are rare at Paris, as they are only produced by an unusual concurrence of various causes. Mr. Lemoine has divided the floods of the Seine at Paris into three classes, denominating floods which rise between 5 and 6 metres (16·4 to 19·7 feet) on the Tournelle gauge as ordinary large floods, those rising from 6 to 7 metres (19·7 to 23 feet) as extraordinary floods, and those above 7 metres (23 feet) as exceptional floods.[1] During the one hundred and fifty-one years which have elapsed between the establishment of daily readings of the river at Paris in 1732 up to 1883, there have been thirty-one ordinary floods, twelve extraordinary, and two exceptional floods, namely, in December 1740 and in January 1802, when the heights attained were 25·95 feet and 24·37 feet, respectively, on the Tournelle gauge. Mr. Belgrand gives a list of five exceptional floods which occurred between 1649 and 1732, namely, in February 1649, January 1651, February 1658, April 1690, and March 1711, of which that of 1658 alone was higher than the flood of 1740, reaching a height of 28·90 feet, being by nearly 3 feet the highest flood on record at Paris. The four other

[1] "Manuel Hydrologique du Bassin de la Seine." A. de Préaudeau, p. 35.

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floods, though lower than the flood of 1740, exceeded the flood of 1802.[1] The flood of 1658 followed on the breaking-up of the ice in the river after a frost which had lasted for five weeks, and was intensified by the thawing of a large quantity of snow accompanied by rain; and the flood of 1740 resulted from prolonged and heavy rains falling upon a large accumulation of melting snow.

From the above records it appears that, whereas four exceptional floods occurred in the latter fifty-one years of the 17th century, only two occurred in the 18th century, and one only has hitherto appeared in the present century. This result indicates, either an inaccuracy in the earlier records, or a freer channel for the flood-waters. Now comparing the floods of the later eighty-three years of the last century with the first eighty-three years of the present century, there have been fifteen ordinary [2] and five extraordinary floods in the former, and twenty-three ordinary and six extraordinary floods in the latter, and one exceptional flood in each. Out of the forty-five large floods recorded since the commencement of the daily observations at the Tournelle gauge in 1732, only three occurred within the warm season, and two of them appeared in May (1779 and 1836), and one in September (1866). This shows that regular floods of the Seine at Paris are almost wholly confined to the cold season; whilst the occurrence of the exceptional instances at the beginning and close of the warm season indicates that they may be traced rather to a slight displacement of the ordinary limits of the cold season than to any peculiar conditions. Two extraordinary floods are, indeed, given by Mr. de Préaudeau, as having taken place in June 1693 and June 1697, the latter reaching the exceptional height of 24·11 feet; but they were ignored by Mr. Belgrand, and throw a doubt on the records of the 17th century.

In the summer season, the rain very seldom rises above 3·50 metres (11·48 feet) on the Tournelle gauge; and Mr. de Préaudeau considers floods rising between 3·50 and 4·50 metres (11·48 to 14·76 feet) as large summer floods, and those exceeding 4·50 metres (14·76 feet) exceptional summer floods. Only ten of these summer floods have been recorded since 1700; and of the four exceptional cases included in this number, one flood does not reach the limit of 5 metres assigned to ordinary large floods. Seven

[1] "La Seine, Études Hydrologiques." M. Belgrand, p. 300.

[2] The recorded number between 1732 and 1800 is 12, so a proportionate number has been added for the previous years.

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of these floods occurred in May, and one in September, thus bordering on the limits of the cold season. Two only occurred in the middle of the hot season; one in June 1757, which rose to 12·96 feet on the Tournelle gauge, and one in July 1816, which only reached 11·78 feet.

Only two unusually high floods have occurred at Paris since 1850, namely, in March 1876, and in January 1883 (Plate 1, Figs. 7 and 13), reaching 21·33 feet and 19·69 feet, respectively, on the Tournelle gauge; though the floods of December 1872 and 1882 were only a little lower, as they rose to 19·19 and 19·16 feet respectively. These two floods, besides being the highest recent floods, possess the special interest of having formed the subject of careful investigations by Messrs. Belgrand and Lemoine,[1] and Messrs. Lemoine and Préaudeau,[2] and of having been the first floods of unusual height to which the system of predictions and warnings was thoroughly and successfully applied. Both floods were due to a succession of floods in the torrential tributaries, the later ones concurring with the floods of the gently-flowing tributaries in raising the water-level at Paris (Plate 1, Figs. 3 to 8, and 9 to 14). The two floods, however, though resulting from a similar combination of causes, like most high floods on the Lower Seine, presented some notable differences. The flood of March 1876 was unusually late in the cold season (Plate 1, Fig. 15), and was produced by an almost continuous rainfall, from the 11th of February to the middle of March, falling upon melting snow at the beginning, and on a soil previously saturated by the winter rains. The river rose almost continuously at Paris from the middle of February till the 17th of March, when it attained its maximum (Plate 1, Fig. 7). The flood of January 1883 was the result of two distinct periods of heavy rainfall following close upon one another, the first extending from the 8th of November to the 9th of December, and the second from the 21st of December 1882 to the 3rd of January 1883. The January flood was accordingly preceded by a high flood in December, which was produced by a greater rainfall than that of February–March 1876, but rose less high at Paris, owing doubtless to the earlier period of the winter season in which it occurred, and to its not being intensified by melting snow (Plate 1, Fig. 13). The January flood was the result of a much smaller rainfall, falling, however, upon an over-saturated soil, and before the previous flood had fully

[1] "Annales des Ponts et Chaussées," 5th series, vol. xiii. p. 435; and also Minutes of Proceedings Inst. C.E. vol. l. p. 221.

[2] "Annales des Ponts et Chaussées," 6th series, vol. vi. p. 314.

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subsided. Accordingly the flood of the 5th of January 1883 exceeded that of the 7th of December 1882, though it did not attain the height of the March 1876 flood at Paris, owing to the break in the continuity of the rainfall. The 1883 flood approached nearer to the maximum of the 1876 flood below the confluence of the Oise, owing to the higher flood of that river in 1883; so that at Mantes (Plate 1, Fig. 15), the flood of 1883 was only 3½ inches lower at its maximum than that of 1876. Lower down the river, the flood of December 1882 actually exceeded the 1876 flood between the bridges of St. Pierre du Vauvray and Rouen, for it rose to 39·43 feet and 20·70 feet on the gauges at the Vauvray and Oissel bridges in December 1882, as compared with 39·17 and 19·82 feet, respectively, in March 1876. This peculiar result was due to the maximum in 1882 coinciding with a high tide, which was not the case in 1876; for the tidal influence during a high tide extends above Martot Weir, which is quite open in flood-time.

The maximum discharge of the Seine at Paris during the flood of March 1876 amounted to 2,171 cubic yards per second, or about forty-seven times its minimum discharge of 46 cubic yards. The maximum discharge of the flood of January 1883 at Paris was 1,967 cubic yards. The discharge at Mantes, during the height of the flood of March 1876, was calculated at 2,943 cubic yards, the minimum summer discharge of the river at that place being about 85 cubic yards per second; whilst at the lock of Meulan, a little above Mantes, the maximum discharge on the 9th of December 1882 was found to be 2,914 cubic yards. The greatest discharge measured on the river during the latter flood was below Elbeuf, beyond the confluence of the Eure, on the 11th of December 1882, and amounted to 3,248 cubic yards per second.

The areas of land inundated between Paris and Rouen were nearly the same in the floods of 1876 and 1882–3, amounting in 1876 to 29,557 acres, and in 1882–3 to 31,371 acres. The flood of 1876 extended over a width of 4 miles in some places, as for instance at Poses. There are only four districts where the land is protected against floods by embankments, so that, for the most part, the floods flow freely over the adjacent meadows. The Venables embankment was breached by the flood of 1876, which increased the inundated area at that part; but though no embankment failed during the flood of 1882–3, the higher rise in the lower part of the river, alluded to above, caused the inundation produced by the latter flood to exceed even that of 1876.

The velocity of propagation of the maximum of a flood differs according to the nature of the flow of the river, being much

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quicker on torrential rivers. It varies also in a river of mixed characteristics like the Seine at different places, and even in different floods according to the changes in the preponderating influences of the various tributaries. Thus the velocity of propagation below Paris, which varied during the flood of 1876 between 1·4 mile per hour at Oissel and 1·9 mile at Garenne, averaging 1·86 mile per hour, amounted to 2·86 miles between Paris and Mantes during the flood of December 1882 owing to the greater influence of the Oise, but was only 1·3 mile per hour in the flood of January 1883; whereas, on the Upper Seine, the velocity averaged 5·72 miles in December 1882, and 3·73 miles in January 1883 between Montereau and Paris. The maximum flood-level took from four and a half to five days in travelling from Clamecy, near the extreme limit of the Yonne basin, down to Mantes, a distance of 238 miles; and four days are generally considered available for predictions of a flood-rise at Mantes.

Predictions of Floods.

The system of predicting the probable height of floods, at certain places on a river, from the indications furnished by the heights attained in the upper tributaries, was inaugurated by Mr. Belgrand for the Lower Seine in 1854. By a series of careful observations, he was enabled to trace a relation between the rise of the various tributaries, at certain points, and the corresponding rise of the Seine at Paris, or lower down. The usual rule adopted for calculating the probable flood-rise of the river at the bridge of Austerlitz at Paris is to double the mean of the rises observed on the Yonne at Clamecy, the Cousin at Avallon, the Armançon at Aisy, the Marne at Chaumont and St. Dizier, the Aisne at St. Menehould, the Aire at Vraincourt, and the Saulx at Vitry le Brûlé. When the rise is preceded by a fall, the mean rise must be multiplied by 1·55; but the results are less exact than in the first case. Another rule is to substitute the Grand Morin at Pommeuse for the Aisne and the Saulx in the above list, which is useful in introducing the influence of the torrential Grand Morin, which exercises a marked effect on small floods, though the small extent of its basin prevents its influence being much felt in high floods. The mean time occupied by the maximum flood-level in travelling from the upper affluents to Paris is three days and four-tenths, and one day from Paris to Mantes. The rise at Mantes is generally the same as at Paris; but when the predicted rise of the Oise at Venette exceeds that of the Seine at Paris, about 1 foot must

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be added for the rise at Mantes. The announcements are generally made three days in advance, and a corrected prediction is issued twenty-four hours later. The heights predicted are usually somewhat in excess of the actual heights recorded, owing to the natural anxiety not to give too low an estimate of the flood; but the discrepancies will tend to diminish with extended experience. Some remarkable coincidences occurred during the two largest recent floods between the predicted and the observed heights of the river. Thus, the height predicted on the 15th of March 1876 for the following day, of 21·33 feet on the Austerlitz gauge, was exactly verified; and the prediction on the 14th of March for the 17th, of 21·98 feet on the same gauge, was recorded at 21·91 feet in the evening of the 17th. On the 26th of November 1882, the probable height of the river on the Mantes gauge was given as 25·26 feet, and the recorded height on the 8th of December was 24·74 feet; and the height predicted on the 5th of January 1883 was 25·26 feet, whilst the height attained on the 7th was 24·93 feet. At Paris, the exact maximum of the flood of January 1883 was predicted the previous evening.

The same system of predictions was established by Messrs. Belgrand and Lemoine for the Aisne, the Oise, the Marne, and the Upper Seine in 1872; but it could not be applied to the higher portions of these rivers, except by means of deductions from the rainfall returns which would furnish somewhat uncertain indications; and, moreover, the warning could only be given so short a time before the arrival of the flood as to be practically useless. Though warnings of approaching floods were principally intended for the benefit of navigation and river-works, they have proved very valuable for the riparian proprietors, who, on the occurrence of the great floods of 1876 and 1882–3, received sufficient notice to enable them to remove their labourers and goods to places of safety, and to raise embankments threatened with submersion.

No extensive works have been undertaken for preserving the valley of the Seine from inundations; but Paris has been protected by raising the quays along the river, and by prolonging the outfall sewers so as to place their outlets beyond the town; and four low-lying districts below Paris have been sheltered by embankments. The rebuilding also of old bridges with more ample waterways, and the improvements in depth for navigation, have increased the discharging capacity of the river. The recurrence, however, of an exceptionally high flood like that of 1658, or even of 1740, would expose portions of Paris to inundation; whilst one of the protected districts was flooded in 1876.

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The floods of the Lower Seine flow so gently over the adjacent lands that they cause little injury; whilst the inundations of the Upper Seine serve as reservoirs for preventing a too rapid descent of the flood-waters on to Paris.

The universal adoption of movable weirs throughout the Seine basin enables the flow of the river to be left unimpeded during winter floods; and the warnings sent to the weir-keepers of the approach of a sudden flood secure its rapid passage through the weirs, without the chance of unduly lowering the water for navigation.

II. INLAND NAVIGATION IMPROVEMENT-WORKS.

The Seine is an extremely winding river, which greatly increases the distance by water between the towns along its banks (Plate 2, Fig. 1). Thus the distance between Montereau and Paris is 42 miles in a straight line, and 60 miles by the river; between Paris and Rouen, 70 miles in a straight line, and 150 miles by the river; and 45 miles from Rouen to the sea in a straight line, and 80 miles by the river and estuary. This large augmentation in the actual length to be traversed, however, though placing the inland navigation at a disadvantage in this respect as compared with railways, has the effect of reducing the fall of the river, which makes the river more stable, reduces the velocity of the current, and renders the canalization of the river easier. The total fall of the bed of the river is about 74 feet between Montereau and Paris (Tournelle Bridge), a distance of 61 miles, and about 76 feet between Paris and St. Aubin, a distance of 135 miles; so the average fall between Montereau and Paris amounts to $14\frac{1}{2}$ inches per mile, whilst between Paris and St Aubin it is only $6\frac{3}{4}$ inches per mile. The fall and the depth vary considerably at different parts, but are more irregular below Paris than above (Plate 2, Figs. 2 and 3).

Like most rivers, the Seine in its natural state consisted of a series of shallows and pools, causing irregularities in the current, which are always unfavourable to the up-stream navigation; whilst in times of drought shoals barred the passage. The only improvements effected on the Lower Seine, before the present century, consisted in narrowing the channel at certain places, called passes, so as to retain a greater depth of water over shoals higher up. The increased current, however, through these passes hindered the up-stream navigation; and forty horses were required for drawing a boat through the old pass of Morue, near Bezons.

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Lower Seine Navigation Works.

The first distinct work of river improvement on the Lower Seine consisted in forming a side channel with a lock at Pont de l'Arche, where the old bridge acted like a weir. This work was commenced in 1804, and completed in 1813. Between 1813 and 1838, the river-works were confined to the improvement and raising of the towing-paths, and dredging shoals. As all the vessels at that time were towed up-stream by horses, great benefit was derived from an improved towing-path; but the removal of shoals proved of little practical advantage, for though the current was thereby rendered more uniform, its velocity was increased, and as the river was lowered fresh shoals appeared. At length, in 1838, the regular canalization of the river was undertaken between Paris and St. Aubin. Mr. Poirée had already successfully tested his system of needle-weirs within the Seine basin by erecting needle-weirs across the Yonne, at Basseville, in 1834, and at Épineau in 1838, for providing an intermittent navigation by means of flushing on that river. He accordingly designed similar weirs, with adjacent locks, for canalizing the Lower Seine, and securing a depth of 5¼ feet. Seven weirs, with their accompanying locks, were erected between 1838 and 1866,[1] dividing the Lower Seine into seven reaches, namely, St. Aubin-Poses, 9¼ miles in length; Poses-Garenne, 25½ miles; Garenne-Meulan, 41¼ miles; Meulan-Carrières, 12½ miles; Carrières-Bougival, 16½ miles; Bougival-Suresnes, 19½ miles; and Suresnes to the Tournelle Bridge, 10½ miles long (Plate 2, Figs. 1 and 2). The river has a width of from 400 to 600 feet between Paris and Épinay; 500 to 750 feet between Épinay and Meulan; and 550 to 980 feet between Meulan and Rouen: and it is subdivided for considerable distances into two or more channels by numerous long islands. Advantage was taken in every instance of these islands for placing the locks and weirs in separate channels, sometimes at points on the river a considerable distance apart, as for instance in the case of Bezons weir and Bougival lock, which are as much as 5½ miles from each other. Mr. Krantz, when proposing fresh works in 1871, stated that the great differences in the lengths of the reaches on such a regular river as the Seine

[1] Bezons weir, with a lock at Bougival, was commenced in 1838; Andrésy weir, with a lock at Carrières-sous-Poissy, in 1846; Notre Dame de l'Isle weir, with a lock at Notre Dame de la Garenne, in 1849; Poses weir and lock in 1850; Meulan weir and lock in 1853; Martot weir with St. Aubin lock in 1862; and Suresnes weir and lock in 1866.

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constituted a notable defect in the above works.[1] From an inspection, however, of the longitudinal section of the river below Paris (Plate 2, Fig. 2), it does not appear that the arrangement of the sites of the locks is open to objection, with the exception of the great gap left between the Garenne and Meulan locks, where the canalization was imperfect, forming an impediment to navigation in dry seasons.

On the completion of Mr. Poirée's navigation-works, it was found that they did not secure, in times of drought, the anticipated navigable depth of 5¼ feet, mainly because, in estimating the available depth in the upper portion of the reaches, credit was given for the inclination of water-level due to the ordinary fall of the river between the weirs; whereas, after a succession of very dry seasons, the discharge of the river fell below its normal amount, and the water-surface became nearly level. This loss of depth was naturally more marked at the upper parts of the reaches; but it must be attributed rather to the error in taking credit for an inclination of water-surface which might cease to exist, than to the unequal distribution of the weirs.

In 1866 fresh works were determined upon, not merely to remedy the deficiencies of the previous works, but also to increase the navigable depth to 6½ feet.[2] These works consisted of an additional lock and weir at Port Villez for improving the depth in the long Garenne-Meulan reach, and the repair and raising of the weirs and locks at Poses, Garenne, and Meulan. Before these works, however, were completed, the need of a still more comprehensive scheme was recognized for providing a greater navigable depth between Paris and Rouen. The completion, however, of the works in progress, and the consideration of extended works, were delayed by the war of 1870–71. At length in 1878 fresh works were authorized, with the object of increasing the depth of the Seine between Paris and Rouen to 10½ feet, so as to secure a navigable channel for vessels of 800 to 1,000 tons burden, drawing 9 feet 10 inches of water. These works consisted in heightening or altering the existing weirs, so as to raise the water-level in the reaches; building larger and deeper locks; erecting a new weir and lock at Méricourt, between Port Villez and Meulan; and

[1] "Amélioration de la Navigation de la Seine entre Paris et Rouen," J. B. Krantz, pp. 12 and 13.

[2] Mr. Poirée's scheme of 1845 was intended to provide a navigable depth of 6½ feet between Paris and Rouen by means of fourteen locks and weirs; but a depth of 5¼ feet was considered adequate at that time, and the scheme was modified accordingly.

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dredging at certain shoal places. The condition of the Lower Seine when all these works shall have been fully completed is shown on the section (Plate 2, Fig. 2). The river is divided into nine reaches, in which the depth is reckoned on the assumption that the water may at times stand perfectly level, so that the chance of an insufficiency of depth, owing to the absence of fall in a dry season, is eliminated; and the only possibility of a reduction in depth below the assigned limit of 10½ feet would be if the loss of water by leakage, evaporation, and lockage, should ever in time of drought exceed the discharge. The falls at the locks are various, ranging between 5¾ feet at Meulan and 13¾ feet at Poses, whilst the average is 9½ feet; the total difference in level between the water-level of the Paris reach, and high water of ordinary neap-tides at St. Aubin, being 85 feet.

On the completion of the above works, the Seine will afford a very commodious waterway for inland-navigation up to Paris. Large sea-going vessels get up to Rouen, which forms the terminus of an important inland trade by water with Paris and other parts of the country. A navigable channel, however, between Rouen and Paris, 10½ feet deep, does not satisfy every aspiration; and the idea of making Paris itself a seaport, which was first conceived in 1825, has been again revived. It is suggested that a tidal channel, with a minimum navigable depth of 19¾ feet, should be excavated along the river up to Poissy, 12½ miles from Paris, where a port would be created, and from whence vessels would be enabled, if necessary, to rise through a chain of locks to a higher reach and proceed right up to Paris.[1] The comparatively small difference of about 50 feet in level between Rouen and the environs of Paris has led to the conception of such an apparently bold proposition, similar to the scheme designed by Mr. Fulton for a tidal ship-canal up to Manchester, but much greater in length, though less in maximum depth. The cost of the scheme has been estimated at £10,240,000. The government has not shown hitherto any disposition to further this scheme; and the Rouen authorities are naturally very strongly opposed to it. It would be wise to wait and see whether the greatly improved waterway between Rouen and Paris, now approaching completion, will not suffice to supply all the requirements of Paris, before attempting to burden the State, already

[1] "Résumé d'une Étude sur la création d'un Port de Mer à Paris." M. Bouquet de la Grye, 1882.
Association Française pour l'Avancement des Sciences, 12e Session, Rouen, 1883, pp. 227 and 230.

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deeply involved in several costly public works, with an enterprise of which the financial advantage is problematical. Rouen appears to be the natural limit of maritime trade on the Seine (Plate 2, Fig. 2); and the improvement of the estuary channel between Berville and Havre (Plate 4, Figs. 1 and 2) is of far more pressing importance than the extension of the tidal river up to a point which, whilst involving a large expenditure and altering greatly the water-levels in the upper valley of the Lower Seine, would not actually attain to Paris.

Upper Seine Navigation-Works.

The improvement of the Seine for navigation between Montereau and Paris, by means of locks and weirs, was only undertaken in 1860, when the treaty of commerce gave an impulse to the methods of communication throughout France. Previously the Upper Seine was quite open; and the only aids to navigation were the flashes coming down from the Yonne and Little Seine, which created a temporary flood, and augmented the depth whilst they lasted. These flashes were produced on the Yonne by rapidly opening the needle-weirs placed right across the river, of which the earliest was erected in 1834; but the small artificial flood produced by them only lasted about twelve hours in the Upper Seine, and only increased the depth from a maximum of 2½ feet at Montereau to 1 foot near Paris. In 1846 it was decided to erect weirs between Nogent and Montereau, so as to provide a similar system of flashes on the Little Seine, which, combined with the flashes of the Yonne, would increase the duration and height of the flood on the Upper Seine. In 1859, it was even proposed to extend the formation of the flashes to the Upper Seine itself by the erection of three movable weirs between Paris and Montereau; but this plan was superseded in 1860 by the decision to abandon the system of intermittent navigation, so prejudicial to the passage of vessels up-stream.

The scheme of 1860 consisted in the canalization of the Upper Seine by the erection of twelve weirs and locks, arranged so as to secure a navigable depth of at least 5½ feet over the lower sills of the locks, with the water standing level in the reach below. The weirs were necessarily placed much closer together than on the Lower Seine, owing to the greater natural inclination in the bed of the river above Paris; and they were so spaced that each weir kept up the water 7 feet 10½ inches above the summer level at its site. Mr. Chanoine, who had charge of the works, designed a

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system of movable shutters for closing the weirs, instead of the needle-weirs erected by Mr. Poirée on the Lower Seine (Plate 3, Fig. 8). In 1878 it was decided to increase the navigable depth from 5½ to 6½ feet. This improvement has been effected by deepening the two top reaches, and lowering the sills of the lock at Varennes, and by raising the water-level in the other ten reaches, which necessitated the reconstruction of the movable portions of the weirs (Plate 3, Figs. 8 and 9). The present condition of this section of the river is shown on Plate 2, Fig. 3. The reaches vary in length from 2⅝ miles between Courbeton and Varennes, to 7 miles between Évry and Ablon; and the falls at the weirs vary between 4½ feet at Vives Eaux, and 8¾ feet at Port-à-l'Anglais, and average 5·6 feet. The change effected by the canalization is readily perceived by contrasting the present summer state of the river, when the water is level in the reaches, with the summer water-level of 1840, when the river was in its natural condition (Plate 2, Fig. 3).

The Oise, the Marne, the Yonne, and other tributaries of the Seine, have been rendered navigable by means of locks and weirs; and canals have been constructed to extend these inland navigations, and to connect them with the waterways of the adjacent basins. The length of navigable waterways within the Seine basin amounted in 1872, according to Mr. Krantz, to 1,585 miles; but he considered that several connecting links within the basin were needed, and also the junction of the Marne and the Saône by means of a canal.

Weirs.

All the weirs on the Seine belong to the general class of movable weirs, though they present various differences in design. In fact, examples of all the principal types of movable weirs are to be found in the Seine basin. The needle-weir [1] was for a long time the sole type adopted on the Lower Seine; whilst only shutter-weirs [2] were erected on the Upper Seine. Drum-weirs [3] have been confined exclusively to the Marne; and the only examples of the bear-trap-weir,[4] the hydraulic shutter-weir,[5] and the segmental gate-weir,[6] are at La Neuville on the Marne, at Brulée Island on the Yonne,

[1] "Rivers and Canals." L. F. Vernon-Harcourt. Plate 5, Fig. 3.
[2] "Fixed and Movable Weirs." Minutes of Proceedings Inst. C.E., vol. lx., Plate 2, Fig. 4, and Plate 3, Figs. 1 and 2.
[3] *Ibid.*, Plate 3, Figs. 3 and 4. [4] *Ibid.*, Plate 2, Fig. 1. [5] *Ibid.*, Plate 2, Fig. 2.
[6] "Rivers and Canals." L. F. Vernon-Harconrt. Plate 6, Figs. 4, 5, and 6.

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and at La Monnaie in Paris, respectively. The largest ordinary frame-weir in France has been erected at Port Villez, on the Lower Seine (Plate 3, Figs. 5 and 6); whilst the first suspended frame-weir has been recently (September 17th, 1885) completed across the Seine at Poses (Plate 3, Figs. 3 and 4). The variety of types adopted within one river-basin has been due to the desire of the engineers in charge of the different sections to devise some new and improved type, and not to the special adaptability of each type to the localities in which they are situated. Thus Mr. Poirée erected needle-weirs on the Lower Seine and the Yonne; Mr. Chanoine placed shutter weirs across the Upper Seine; Mr. Desfontaines employed drum-weirs on the overfalls of the Marne; and Mr. Caméré is occupied in the construction of frame-weirs, suspended from overhead girders, for the new weirs on the Lower Seine.

The different kinds of movable weirs have been so fully described in the Author's Paper on "Fixed and Movable Weirs," read before this Institution in 1880,[1] that a brief description will suffice of the general arrangement of the Seine weirs, together with some account of the modifications and improvements recently effected.

Upper Seine Weirs.—The weirs on the Seine are always composed of two distinct sections; namely, a navigable pass, used by vessels when the river is high and the lock is submerged; and a regulating weir with a sill raised above the river-bed, which, having consequently a less height, can be more readily opened for the passage of a moderate flood, and is more convenient for regulating the discharge than the larger weir of the navigable pass.

On the Upper Seine, the navigable pass is placed between the lock, situated alongside the towing-path bank, and the regulating weir stretching from the river pier of the navigable pass to the opposite bank, as shown by the plan of Ablon weir (Plate 2, Fig. 7). The navigable passes on the Upper Seine vary in width from 132½ feet at Varennes and La Madeleine, to 213½ feet at La Cave, and their average width is 165 feet; whilst the regulating weirs vary from 124 feet at Melun, to 230 feet at the four lowest weirs. The sills of the passes are 3¼ feet lower than the sills of the regulating weirs; and formerly both the passes and overfalls were closed by Chanoine's shutters.

In the recent improvement works, higher shutters have been placed on the old sills of the navigable passes, at the lower end of those reaches whose water-level had to be raised 1⅓ foot.

[1] Minutes of Proceedings Inst. C.E., vol. lx. p. 24.

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These shutters resemble in principle the shutters they have replaced, but they have been improved in certain details (Plate 3, Fig. 8). The old shutters were liable to be fractured by the shock of their fall on being lowered; they were only raised to within 3 inches of the proper upper water-level, so that special contrivances were required to retain the full height of water in times of drought; a space of 4 inches was left between the shutters; and the shutters, having their axis of rotation situated one-twelfth of their height below the centre, opened of their own accord when the upper water-level rose from 4 to 8 inches above them, and when the tail water was $6\frac{1}{2}$ feet deep, which often happened in rainy years. The new shutters have been made stronger; their crest rises to the proper water-level, 11 feet 2 inches above the bottom, their length on the slant being 11 feet 8 inches; their width has been increased to 4 feet 1 inch, reducing the interval between each shutter to 2 inches; and the axis of rotation has been placed only $1\frac{1}{3}$ inch below the centre, which secures the shutter from opening spontaneously. Moreover, a butterfly valve, $3\frac{1}{4}$ feet high and 2 feet wide, has been placed in the upper leaf of each shutter, which serves to regulate the water-level, to aid the passage of small floods, to ensure the safety of the shutters during larger floods, and to facilitate the raising of the shutters.[1]

The shutters across the regulating weirs have been only retained where the water-level had not to be raised. In all other cases the shutters have been replaced by needle-weirs (Plate 3, Fig. 9). The needles, $11\frac{1}{2}$ feet long and 3 inches square, have been furnished with hooks, so that they can revolve on the top front bar of the foot-bridge, which greatly facilitates their working.[2]

Needle and shutter weirs are accordingly now found side by side on the Upper Seine, and both appear well adapted for a river with a fairly regular flow. Needles are simpler in construction, more easily moved, and less liable to get out of order than shutters; but they require the constant attention of the weir-keeper for regulating the flow. Shutters allow of a moderate flood passing over their crest, and their extent of opening can be adjusted beforehand by their chains. The new weirs on the Upper Seine constitute a notable advance in the facility, rapidity, and security of working of these types of weirs.

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[1] Details of these recent improvements will be found in Annales des Ponts et Chaussées, 6th series, vol. v. p. 622, and plate 16.

[2] A description of this contrivance is given in Annales des Ponts et Chaussées, 6th series, vol. ii. p. 220; and in abstract in Minutes of Proceedings Inst. C.E., vol. lxvii. p. 453.

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Suresnes Weir.—The weir at Suresnes, a short distance below Paris, which formerly was a needle-weir precisely similar in construction to Martot weir,[1] has been entirely reconstructed, in order to provide a minimum depth of 10½ feet as far up as Bercy by raising the water-level 3¼ feet. Formerly there was a weir adjoining the lock, another weir opposite Neuilly connecting Rothschild Island with another island in mid-stream, and a third weir between this island and the left bank at Levallois, 2½ miles below the lock. The great distance between the weirs was very inconvenient for working, and the raising of the water-level would have occasioned infiltrations through this long length of narrow islands. Accordingly the weirs have been all placed close to the lock, across the three channels formed by Rothschild Island and the small Folie Island, as shown on Plate 2, Fig. 5. The system of frames has been retained on an enlarged scale, the frames across the navigable pass being about 20 feet high (Plate 3, Fig. 2), in place of 11 feet as at Martot. The system of needles has, however, been abandoned, as the needles requisite for such a height would be too heavy to be moved conveniently: the central regulating weir is closed by sliding panels, as designed by Mr. Boulé, and previously adopted at the Mulatière weir at Lyons, and at six weirs on the river Moskwa; whilst the two side-passes are closed by Mr. Caméré's hinged curtains, first adapted by him to the Port Villez weir (Plate 3, Figs. 5, 6, and 7). These two systems of closing large frame-weirs are accordingly being tested side by side at Suresnes. The sliding panels are cheaper and simpler in construction, and they let the water fall over the top, the upper ones being raised first. The hinged curtain, being rolled up from the bottom, makes the water escape below; it admits of a better adjustment of the discharge through the weir, and is not liable to be impeded by floods in the act of raising. The sliding-panel, being submerged, is less easily worked, but the hinged curtain causes scour at the bottom; the one system has the merit of simplicity and economy, the other possesses the advantage of more regular working. Time will demonstrate their relative durability, and whether the cheapness of the one more than compensates for the efficiency of the other.

The works at Suresnes were completed towards the close of 1884. Mr. Boulé, the Engineer-in-Chief of the works, thinks that a navigable depth of 14¾ feet, instead of 10½ feet, could have been realized with a not much greater expenditure of time and money;

[1] A section of Martot weir is given in "Rivers and Canals," Plate 5, Fig. 3.

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and he considers it possible that these new works may not exceed in duration the works of 1866–67, which they have replaced, in view of the prospect of extended improvements in weirs and increased requirements for navigation.

Port Villez Weir.—The weir at Port Villez, near Vernon, unlike the older weirs on the Lower Seine, has been carried across the river where it is quite clear of islands, so that the weir forms a continuous line from the lock on the left bank over to the right bank. The total length of the weir is 700 feet. The weir consists of two central navigable passes, and a regulating weir on the right side of the river, which are separated by two intermediate piers (Plate 3, Fig. 5). The sills of the navigable passes are situated 13 feet 1½ inch below the upper water-level, and the sill of the regulating weir is only half that depth down. The weir is closed by frames of the Poirée type, supporting a series of Mr. Caméré's hinged curtains (Plate 3, Figs. 5, 6, and 7). The frames are placed 3 feet 7¼ inches apart; those across the navigable passes are 18 feet high, and weigh 37½ cwts., and those on the overfall are 9 feet 2 inches high, and weigh 7½ cwts. The tops of all the frames are 1¾ foot above the upper water-level, and carry a foot-bridge and a small tramroad, upon which the carriages for conveying the curtains run (Plate 3, Fig. 7). The weir cost £163 7*s.* per lineal foot. Works were begun in 1869 with a view to the erection of a pontoon-weir, according to a novel design by Mr. Krantz;[1] but a trial made of the system at Bougival having proved unsatisfactory, the design was abandoned, and the frame-weir eventually adopted was commenced in 1876. Since its completion in June 1880, it has satisfactorily fulfilled all the requirements as regards navigation. The curtains are worked with ease, and have proved both watertight and durable. The frames have not proved equally satisfactory, for, owing to their great height and weight, and the silting up of their resting-place in the river-bed, they are liable to be deflected, and even to be broken when lowered. The introduction, however, of a curve in the surface of the apron just above the weir (Plate 3, Fig. 6) keeps the floor of the frames clearer of silt, and has consequently reduced the chance of injury. Each frame can be raised or lowered in ten minutes; and the same time is occupied in rolling up or unrolling a curtain; whilst six minutes and two-thirds, on the average, suffice for conveying a curtain to its destination. The whole operation of opening or closing the weir completely occupies two and a half days, when a gang of four men is em-

[1] Minutes of Proceedings Inst. C.E., vol. lx. p. 38, and Plate 3, Figs. 5 and 6.

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ployed at the frames and another similar gang at the curtains, during the daytime only. This rate of work suffices for the Seine, but it evidently could be readily accelerated by working at night, or by employing more gangs.

Poses Weir.—Formerly the weirs of Poses were situated about ½ mile apart, and more than a mile above the lock. The new weir has been erected across a wide branch of the river close to the locks, and only separated from them by the narrow end of an island (Plate 2, Fig. 4). This weir has been constructed on an entirely novel principle; for though a system of lifted frames was suggested in 1873 by Mr. Tavernier for the Saône, Poses weir, designed by Mr. Caméré, is the first instance in which all the movable parts of a weir can be removed completely from the river-bed. The weir is of the frame type, and is closed with hinged curtains, like that at Port Villez; but the frames, instead of being fastened on to the apron and lowered on to the river-bed in flood-time, are suspended from an overhead girder, and, whilst their ends rest against the sill of the weir, when hanging vertically in place, they can be raised in flood-time to a horizontal position entirely out of the river (Plate 3, Fig. 3). This arrangement secures the frames from any chance of injury when the weir is open, and it greatly facilitates the working and maintenance of the frames. It, however, necessitates the adoption of long frames for the navigable passes, so that the frames when raised may afford a sufficient headway underneath for the passage of vessels in flood-time, and also overhead girders resting on high piers for supporting and lifting the frames. The great increase entailed thereby in the size of the works is most forcibly illustrated by Plate 3, on which the different types of weirs have been drawn to the same scale for the sake of comparison. Even Suresnes and Port Villez weirs, and the shutter-weirs of the Upper Seine (Plate 3, Figs. 2, 6, and 8), though the largest weirs of their class, sink into insignificance when compared with the hinged frame-weirs of Port-Mort and Poses (Plate 3, Figs. 1 and 3).

The weir at Poses consists of seven openings separated by piers (Plate 2, Fig. 4), of which the two adjoining the left bank are navigable passes, 106⅓ feet wide, with sills 16 feet 5 inches below the upper water-level, and leaving a clear headway of 17¼ feet above the highest navigable flood-level, 1½ foot higher than the ordinary level, but 3⅛ feet below the flood of 1876 (Plate 3, Figs. 3, and 4). The three passes nearest the island are each 99 feet in width, and have their sills at the same level as the navigable passes; but their overhead girders are placed 8 feet lower in level

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than those of the navigable passes. The two central openings are also 99 feet wide, and have their girders at the same level; but their sills are 6⅔ feet higher than the rest of the weir (Plate 3, Fig. 4). There is a foot-bridge across the weir, and the levels of the girders are so arranged that the platform between the girders over the navigable passes is level with the platform on the top of the girders across the other passes, so that the roadway of the bridge is at a uniform level throughout. Four verticals are braced together so as to form a single frame occupying 7 feet 7½ inches in width of the weir. A hinged plate at the back of each frame provides a small foot-bridge at the back of the weir, above the highest navigable flood-level, from which the curtains are rolled up or unrolled by aid of a crab (Plate 3, Fig. 3). The curtains are precisely similar in design to those of Port Villez, but they have been doubled in width, being 7½ feet wide, with a clearance of 1½ inch between them. The curtains are not removed when rolled up for opening the weir, but they remain on the frame whilst it is being raised by chains from the bridge, and even after it has reached its horizontal position, unless needing repairs (Plate 3, Figs. 1 and 3). The rolling up or unrolling of a curtain of one of the deep passes occupies fifteen minutes; whilst the raising of a frame occupies twenty minutes, and its lowering ten minutes. Consequently one of the passes could be entirely opened in five hours; and the whole weir would require from five to thirty-five hours for opening, according to the number of gangs employed.

The weir was put into operation in September 1885. It cost £151 5*s.* per lineal foot, being less than that at Port Villez, notwithstanding the overhead girders, high piers, and long frames, as well as the greater head of water retained. The smaller cost of an apparently much larger work must be partly due to the less strength required for the frames, which are only exposed to the direct pressure of the water when the weir is closed, to the simpler apron in the absence of hinges at the bottom for the frames, and to the saving of the cost of modifications in the foundations and frames, and of experiments on the novel system of curtains, which were carried out at Port Villez weir. As the only objection that could be raised against the Poses type of weir would be the cost of so large a work, it is evident that if the expenditure on Poses and Port Villez weirs may be accepted as a fair comparison of the respective cost of the two systems, the suspended frame-weir is decidedly superior to the ordinary frame-

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weir for heads of 10 feet and upwards. For whilst the suspended frame-weir is apparently not more costly than the ordinary frame-weir in these cases, it is more quickly raised or lowered, and more easily maintained. Moreover, this system provides a permanent foot-bridge, and it could readily be adapted for greater heights by merely having longer frames and higher piers.

Port-Mort Weir.—The intervening weir between Port Villez and Poses was formerly situated across the right branch of the Seine, at Notre Dame de l'Isle, about 2 miles above the lock of Notre Dame de la Garenne. The site of the weir has been changed, like at Suresnes and Poses, so as to place it near the lock; and the new weir is being constructed across the right branch of the river, in a line with the old needle-weir adjoining the lock (Plate 2, Fig. 6). The needle-weir has simply been heightened, so as to retain a raised water-level of $1\frac{1}{2}$ foot. The new weir in course of construction consists of seven passes, each about 99 feet wide, separated by piers 13 feet in width, and having sills 13 feet 1 inch below the upper water-level. It is very similar in design to Poses weir, differing only from it in a few minor details (Plate 3, Fig. 1). The verticals are braced together in pairs, each frame being separated by an interval nearly equal to its width, so that each curtain, having the same width as at Poses, will overlap its frame on each side, and meet the adjacent curtains in the centre of the intervals between the frames. The overhead girders are all placed at the same level, so that when the frames are raised there is a clear headway of $16\frac{1}{4}$ feet above the highest navigable level, as at the navigable passes at Poses. Owing, however, to the much greater height of the navigable level above the normal level, the frames have to be longer, and the piers higher than at Poses, though the normal head is $4\frac{1}{3}$ feet less. (Compare Plate 3, Fig. 1 with Fig. 3.) The difference between the upper and lower water-levels is less at Port-Mort than at Suresnes and Port Villez, or even than at the Upper Seine weirs, so probably a cheaper form of weir might have been adopted in this instance.

Meulan and Méricourt Weirs.—The improvement-works comprise the reconstruction of Meulan weir, and the construction of an additional weir at Méricourt between Meulan and Port Villez.

Meulan weir is situated where the river is separated into two channels by an island; and the new weir is designed to have two navigable passes, having a total width of 243 feet, and a shallow pass, 115 feet wide, across the left branch of the river and adjoining the locks on the left bank; whilst another weir stretches in the same line across the right branch, having a width of 256 feet.

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Méricourt weir, in course of construction, extends in an unbroken line from the locks on the left bank to the opposite bank. It consists of two navigable passes adjoining the lock, having a total width of 213 feet, and two shallower passes with a total width of 485 feet.

The navigable passes of both these weirs are to be closed by suspended frames supporting hinged curtains, resembling Port-Mort weir; whilst the other portions of the weirs will have ordinary frames with curtains, like Port Villez weir.

Bezons, Andrésy, and Martot Weirs.—The two weirs between Suresnes and Meulan, at Bezons and Andrésy, and the lowest weir on the Seine at Martot, require so little modification to provide the increased depth, that it is not proposed to change their sites, although Martot weir is ½ mile away from St. Aubin lock, and Bezons and Andrésy weirs are a considerable distance from their corresponding locks, at Bougival and Carrières-sous-Poissy (Plate 2, Fig. 1). As the water-level of the Bougival-Suresnes reach is to remain unaltered, no raising of the needle-weir at Bezons is necessary; but it is proposed at some future time to lower the sill of the navigable pass 3½ feet, so that it may be 13 feet below the upper water-level.

The water-level of the Carrières-Bougival reach has been raised 10 inches by lengthening the needles of Andrésy weir and raising the foot-bridge on the frames 1½ foot. The sills of the navigable passes are eventually to be lowered 2½ feet, so as to obtain a depth of 13 feet over them.

Since the water-level of the St. Aubin-Poses reach is not to be altered, the needle-weir at Martot will need no modification, except at the navigable pass, when the proposed lowering of its sill 3½ feet is carried out.

Locks.

Upper Seine Locks.—The twelve locks on the Upper Seine adjoin the weirs (Plate 2, Figs. 3 and 7). Eleven of these locks have pitched slopes of 1 to 1 at the sides; they are 55¾ feet wide at the surface-level of the lower pool, which is 6½ feet above the bottom, and the available length of the lock-chamber is 564 feet. The lock at Port-à-l'Anglais, just above Paris, has vertical side-walls; its chamber has an available length of 590 feet, and a width of 52½ feet. The entrance width of these locks is 39½ feet.

Lower Seine Locks.—Previous to the commencement of the works, authorized in 1878, for increasing the depth of the Lower Seine to

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10½ feet, there were single locks at each of the weirs, with lock-chambers about 394 feet long and 39⅓ feet wide. At Bougival, where the traffic is the greatest, the old lock has an intermediate pair of gates. The new works comprised the construction of enlarged locks, with their lower sills 10½ feet below the water-level of the lower pools, supplemented in most instances by a smaller lock alongside, to prevent unnecessary waste of water in locking smaller vessels. The lock-chambers of the large locks are 462 feet long and 55¾ feet wide, and of the small locks 197 feet long and 27 feet wide. The available entrance width, however, at the gates of the large locks is retained at 39⅓ feet, as in the old locks. The lock-chambers at Suresnes (Plate 2, Fig. 5), and Bougival have been given the greater lengths of 526 and 722 feet respectively. Some of these locks have been completed, as, for example, at Suresnes, Bougival, Carrières, and Meulan, and the remainder are either in process of construction or are projected. In some cases three locks will in future supply the requirements of navigation, as, for instance, at Bougival, Garenne, Poses, and St. Aubin (Plate 2, Figs. 4 and 6); but Meulan and Port Villez will be served by the old lock and the new large lock only, as no provision has been made for a small lock; and at Suresnes, the small lock has been added in prolongation of the old lock (Plate 2, Fig. 5); whilst at Méricourt there will only be the large and small locks of the new type, as no lock has existed there before.

Cost of Inland Navigation-Works.

In considering the cost of the works for improving the navigable capabilities of the Seine, it must be borne in mind that the works on the Upper, and still more on the Lower Seine, as they exist at present, are the results of successive experimental trials and alterations, so that the total expenditure has been much greater than would be required for the actual erection of the existing works.

Cost of Upper Seine Works.—The twelve weirs and locks between Montereau and Paris cost originally £432,500, or about £36,000, on the average, for each weir and lock. These works, completed in 1873, increased the navigable depth from less than 2 feet in dry weather up to 5¼ feet. The lock at Port-à-l'Anglais was deepened in 1869, and a new navigable pass formed in 1870, at a cost of £23,880, which is included in the amount given above. The lock cost about £16,000, out of the total of £36,000; the navigable pass, retaining a depth of water of 10 feet on its sill, cost £37 10*s*.

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per lineal foot; and the regulating weir, with its sill 3½ feet higher, cost £21 15*s.* per lineal foot, including £4 12*s.* for the foot-bridge on movable frames added in 1869. The deep pass at Port-à-l'Anglais, with a depth of 13 feet on its sill, and a foot-bridge, cost £62 6*s.* per lineal foot.

The works for raising the navigable depth of the Upper Seine from 5¼ to 6½ feet have involved an additional expenditure of £240,000 between 1878 and the end of 1884. These works are now completed; and Mr. Boulé considers that a further sum of £47,500 would amply suffice for these weirs for some years to come, which would raise the total expenditure on the twelve weirs and locks to £720,000, or an average of £60,000 for each weir and lock. Mr. Lavollée estimates the annual cost of maintenance at £3,480 on the average.

Cost of Lower Seine Works.—Some of the weirs and locks comprised in the improvement-works authorized in 1878 are as yet unfinished, so that the total cost of the works cannot be ascertained. A sum of £412,000 was allotted in 1846 for the execution of the works for ensuring a navigable depth of 5¼ feet. In 1866 a further expenditure of £260,000 was decreed. Mr. Krantz stated, in his Report to the National Assembly in 1872, that the total expenditure on the Lower Seine since the beginning of the century had amounted to £1,058,000; but of this, only £657,300 had been expended on weirs and locks, the remainder having been devoted to dredging, protection of banks, towing-paths, embankments, and accommodation for vessels. The estimated cost of the new works for securing a depth of 10½ feet, authorized in 1878, was £1,280,000; and a further expenditure of £420,000 was allowed in 1880 for the special purpose of extending this increase in depth from the Concorde Bridge right through Paris. This raises the total estimated expenditure on weirs and locks in the Lower Seine to £2,372,000.

Various details of the actual and estimated costs of the new works are given in Mr. de Lagrené's report for 1881,[1] from which it appears that the cost of the lock and weir at Port Villez, including repairs of the frames and alterations in the sills of the passes, amounted to £234,940; the Bougival locks cost £112,800; the Carrières-sous-Poissy locks and cut, £104,600; the Meulan lock and cut cost £95,430; and the estimated cost of the weir and locks at Méricourt was £240,000.

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[1] Navigation de la Seine, 3me Section. Rapport de l'ingénieur en chef pour l'inspection de 1881, pp. 121–138.

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NAVIGATION.

Upper Seine Navigation.—The width of the navigable channel of the Upper Seine varies from 115 to 164 feet. The vessels navigating this section of the river convey loads of from 100 to 350 tons of merchandise; they go in trains, for, as the trade is mainly towards Paris, the same tug which conveys the loaded vessels down-stream can tow the vessels when empty up-stream. Traction by horses or men has almost disappeared on the river since the improvements in depth, and the vessels are towed either by a tug hauling on a sunken chain or by an ordinary steam-tug.

The traffic on the 60½ miles of the first section of the Seine, between Montereau and the entrance to the St. Martin Canal, which Mr. Krantz reckoned at about 1,245,000 tons of merchandise in 1868, and which did not exceed 1,500,000 tons before the increase in depth from 5¼ to 6½ feet, reached 2,100,000 tons in 1882, and 2,300,000 tons in 1883, showing the impulse given to the river traffic by the improved facilities for navigation.

Lower Seine Navigation.—The tonnage of the vessels regularly navigating the Lower Seine varies from 250 to 500 tons; but when the river is high, vessels of nearly 800 tons pass along, and as soon as the works in progress are completed, the river will be available for vessels of 1,000 tons. Comparatively few vessels are now towed by horses, and only those of the smallest class. Traction is generally conducted on the Lower Seine by one of the following methods: steamers carrying cargo; steamers both carrying cargo and towing; steam-tugs; portable steam-propellers fixed at the stern of the vessel; and tugs hauling on an immersed chain.

The Lower Seine is divided into two sections, namely, the second section of the Seine, from the end of the first section, at the entrance of the St. Martin Canal, down to the mouth of the Oise just above Andrésy weir, a distance of 25¾ miles; and the third section, from the mouth of the Oise to Rouen, having a length of 107 miles (Plate 2, Fig. 1). Owing to the considerable amount of traffic brought into the second section from the Upper Seine and the Oise for Paris, and the imperfect canalization of the third section, the traffic per ton-mile in the third section was only a little in excess of that in the second section in 1868, in spite of the great difference in their lengths, amounting to 53,334,000 ton-miles in the second section, and 58,421,000 ton-miles in the third section. By 1878, however, the traffic in the second section was somewhat reduced, whereas the traffic in the third section had increased

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amounting respectively to 49,504,000 ton-miles and 68,033,000 ton-miles. In 1881, the traffic per ton-mile in the second section again approximated to that of the third section, being 67,656,000 ton-miles as compared with 71,351,000 ton-miles in the third section, owing probably to the development of traffic from the Upper Seine. The traffic, however, of the third section has made a rapid advance since 1881, having attained 84,713,000 ton-miles in 1884.

Hydraulic machinery has been provided at the Bougival locks for working the gates and sluices, and for the haulage of vessels through the locks, because a considerably larger number of vessels pass through these locks than through any of the other locks on the Lower Seine. This is due to the passage through the Bougival locks of all the vessels from the Oise bound for Paris, as well as those coming up the Seine; whilst some of them afterwards diverge into the St. Denis Canal below Suresnes. The traffic through the locks at Suresnes, though notably less than at Bougival, is far ahead of that through the locks below Bougival.

The navigation of the river is free both by day and by night, no tolls being charged, for the lock-keepers are paid by the State, and are bound to pass vessels at any time when required, receiving an extra payment for each lockage effected at night. The locks and weirs are lighted at night with coloured lamps so as to indicate the navigable channel, and some of the bridges are also lighted for navigation; and special regulations exist relative to the lights to be exhibited at night by vessels on the river.

III. ESTUARY-WORKS.

The improvement-works carried out on the tidal Seine are much simpler in character than the inland-works previously described, for they consist merely of mounds of rubble chalk forming longitudinal training-walls on each side of the channel; but they have produced most important changes in the navigable channel, and in the estuary beyond (Plate 4, Figs. 1 to 7, and Plate 5, Figs. 1 to 4).

The position of Rouen naturally marked it out for an inland port, for whilst situated near the head of the tidal Seine, it possesses water-communication with Paris. Whereas, however, the river has a stable bed in rock, clay, and gravel, with a good natural depth for 39 miles down to La Mailleraye, about half-way between Rouen and the sea; below this point, it flowed in a shallow, winding, changing channel through shifting sand-banks to the sea (Plate 5, Fig. 1). The shoals rose so high at Aizier and Villequier

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that the depth of water over them at high-water spring-tides was about 10 feet, so that only vessels of from 100 to 200 tons could go up the river, and they frequently had to wait for a high enough tide. Four days at least were occupied in the voyage between the sea and Rouen; and the perils of navigation were aggravated by the bore, caused by the impediments offered by the shoals to the flood-tide in its passage up the estuary, and consequently wrecks frequently occurred. The charge for freight was 8*s*. per ton, and the rate of insurance was $\frac{1}{2}$ per cent.

From the time of Louis XV. great anxiety was manifested for remedying the perils of the estuary, and numerous schemes were put forward by various engineers for the improvement of the river. Mr. Cachin, engineer of the Cherbourg breakwater, about the year 1770, proposed to avoid the estuary altogether by constructing a ship-canal along the left bank from near Villequier down to Honfleur. Others suggested the improvement of the river by groynes or by weirs; whilst Mr. Frimot proposed to narrow and train the river by two rows of caissons, along each side, filled with stones; and Mr. Bleschamps conceived the idea of substituting simple rubble longitudinal training-walls. Nothing, however, was accomplished till, in 1845, Mr. Bouniceau designed a plan for the improvement of the estuary by rubble training-walls from La Mailleraye to Havre, backed up by forcible arguments for its adoption[1] (Plate 4, Figs. 1 and 9). In the following year, 1846, the improvement of the navigation between Villequier and Quillebeuf, by this method, was authorized; the left bank was commenced at Belcinac Island, opposite Villequier, in 1848, and the training-works reached Quillebeuf in 1851. These works immediately increased the depth at the shoals from 10$\frac{1}{2}$ to 21$\frac{1}{3}$ feet at spring-tides. Accordingly the extension of the training-walls was decreed, in 1852, from Villequier up to La Mailleraye, and from Quillebeuf down to Tancarville and La Roque (Plate 4, Fig. 1). The dredging also of the Meules bank, a little below La Mailleraye, was undertaken at the same time. The upper works were completed in 1856, the extension to Tancarville on the right bank having been finished in 1855; but the extension on the left bank to La Roque, with a cross bank from the point to its extremity in order to protect it, was not terminated till 1859. The channel between the embankments was made 980 feet wide at Villequier, increasing to 1,310 feet at Quillebeuf, and to 1,640 feet at Tancarville. The above works provided a deep, stable channel

[1] "Étude sur la navigation des rivières à marées." M. Bouniceau, p. 152, pl. 2.

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down to Tancarville. Below this point, however, the channel diverged from the southern embankment towards the north, and then turned southwards to Berville, and it was also shallow and changeable. Moreover the bore, which had been greatly reduced since 1854, reappeared with all its former power at the close of 1858, and did considerable damage to the retaining-walls. A prolongation of the northern training-wall was accordingly authorized in 1861 from Tancarville to La Roque, and the repair of the banks was commenced. The northern training-wall was completed to La Roque in 1863; and a further extension of the walls to Berville was decreed, which was completed in 1869 (Plate 5, Fig. 2). A prolongation of the southern bank for 1,300 yards below the mouth of the Rille was carried out in 1869–70; but a proposed prolongation of the northern bank in 1871, for a distance of 1¼ mile, was refused on behalf of Havre. The embankments, therefore, remain now at the points they had reached in 1870, except that most of the bank below the Rille has become covered with sand, and considerable repairs are being executed. The actual lengths of the training-walls are given below, for they do not correspond with the embanked channel, since the river flows in some parts close along its natural bank.

——	La Mailleraye to Caudebec.	Caudebec to La Vaquerie.	La Vaquerie to Quillebeuf.	Quillebeuf to La Rille.	Below La Rille.	Total.
	Yards.	Yards.	Yards.	Yards.	Yards.	Yards.
Left bank . .	6,671	10,390	1,006	15,311	1,312	34,690
Right bank . .	6,835	9,077	13,670	15,311	..	44,893

The total length of the training-walls is therefore about 45 miles.

The original training-walls consisted simply of rubble chalk quarried from the cliffs bordering the river in places. The mound was formed by tipping in the chalk at random below low-water, and placing the blocks, filling up the interstices, and trimming the slopes above low-water; it was made about 6½ feet wide at the top, with a slope of 1 to 1 on the land side, and slopes varying from 1½ to 1 up to 8 to 1 in exceptionally exposed places on the river side (Plate 4, Fig. 3). Piling also was placed in some parts along the toe of the river slope. The cost per cubic yard of mound put in place amounted to about 1*s.* 0½*d.* The mounds were raised to high-water level down to Tancarville on the north, and to La Roque on the south, except between La Mailleraye and Caudebec on the south side, where the bank was kept low for the

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sake of economy. Below Tancarville and La Roque low banks were adopted by Mr. Emmery, raised only 3¼ feet and 1⅔ foot respectively above low-water of spring-tides, so that they might have less influence in promoting the accumulation of deposits, which had already become a serious consideration.

The bore, which at some points causes a rapid rise in the river of about 10 feet (Plate 4, Figs. 10 and 11), and appears in places as a breaking wave, together with the deepening of the river-bed by the training-works, disturbed and undermined the training-walls, so that they required raising (Plate 4, Fig. 4). The simple addition of rubble chalk on the top of the old mounds was pursued for several years; but these banks could not efficiently resist the action of the bore. Accordingly, in 1878, a new type of mound was adopted, the river slope being protected by a facing of concrete from 8 to 12 inches thick, and secured where necessary from undermining by a concrete apron, 15 inches thick and 10 feet wide, with piling and planking at its outer edge (Plate 4, Figs. 5 and 6). The training-walls are being gradually reconstructed in this manner, which secures them against the bore. The low south training-wall below La Roque experienced such injuries from the rush of the tide over it at certain periods, that it has been raised to mean high-water level, as shown in Plate 4, Fig. 7; and the north wall below Tancarville must be similarly dealt with.

The total cost of the works up to 1876 amounted to £678,060; in 1878 a sum of £144,000 was allotted for the reconstruction of the banks; and it was proposed in 1883 to expend a further sum of £376,000 in completing these repairs.

The training-works have effected a very remarkable increase in the depth between La Mailleraye and Berville, as indicated by comparing the longitudinal sections of the channel in 1824 and 1875 (Plate 4, Fig. 2). The minimum navigation-depth between Havre and Rouen, at high-water neap-tides, has been increased from 10 feet up to 18 feet. Vessels of 2,000 tons can navigate the river; and it has been found possible, under favourable conditions, to bring up a vessel drawing 21 feet. Moreover, the charge for freight between Havre and Rouen has been reduced to one-half; and the rate of insurance for Rouen is the same as for Havre. The trade of Rouen has consequently greatly increased, and it has become the fifth port of France. The Meules bank is the shallowest point below La Mailleraye, but it could be still further lowered by dredging.

Considerable alterations have been effected in the river-channel at Rouen, but mainly with the object of increasing the quay

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accommodation of the port. These works consist in regulating the river where it is intersected by islands, so as to form one enlarged main channel, by modifying the lines of the banks, and joining the islands to the mainland. By this means, the wasted area of the islands is available for quay space; and the regulated banks are lined with quay walls.[1] The shallow parts of the river between Rouen and St. Aubin are to be dredged, so as to provide a minimum depth of 13½ feet at high-water neap-tides for the up-river navigation.

The range of spring-tides, which amounts to 23 feet 7 inches at the mouth of the Seine, is reduced to about 10 feet at La Mailleraye, and to 6 feet 7 inches at Rouen. The variations in the tidal rise and flow at different places on the river are clearly indicated by the tidal diagrams (Plate 4, Figs. 10, 11, and 12). The improvement effected in the channel by the training-works has caused a lowering of the low-water at Rouen by 2 feet 3½ inches at spring-tides, and 2 feet 9½ inches at neaps; whilst the high-water level has remained unaltered. The tide reaches Rouen fifty-eight minutes earlier than formerly, namely about two and a quarter hours after high-water at Havre; whilst high-water at Rouen occurs from five and a quarter to six and a quarter hours later than at Havre.

The training-walls have more than fulfilled the expectations of their effects on the depth of the channel between them; but their deepening influence extends very little beyond their extremities. Nevertheless, by bringing the deep channel of the river much nearer the sea, the intervening gap of shallow channel in the estuary is readily traversed at high-water in fine weather. Moreover, owing to the greater range of tide in the lower estuary, the depth near high-water in the navigable channel through the estuary approximates to that within the trained channel, so that little inconvenience would be felt from the absence of an improved depth between Berville and the sea if the estuary channel was stable (Plate 4, Fig. 2). This channel, however, is constantly shifting, and though it is carefully marked by buoys, the changes cannot be immediately indicated during stormy weather, when the greatest alterations occur, and when a knowledge of the exact position of the channel is most urgently needed (Appendix I.). Also in fogs, an altered channel, even though marked by buoys,

[1] A description and plan of these works will be found in "Harbours and Docks," L. F. Vernon-Harcourt, pp. 428 and 619, and Plate 13, Fig. 2, and Plate 15, Fig. 15.

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is not readily discovered. Accordingly vessels incur considerable risk at times, and are occasionally wrecked in traversing the interval between the sea and Berville. The growing trade of Rouen, and the improvements in progress above, enhance the importance of a safe and easy channel between Rouen and the sea. The shiftings of the estuary channel can only be prevented by an extension of the training-walls, which would also increase the depth.

The training-walls have been executed very nearly in accordance with the lines indicated by Mr. Bouniceau in 1845, but they have not been carried out to the extent he proposed. The Author pointed out in his Paper on "Harbours and Estuaries"[1] that Mr. Bouniceau had considered accretion in the estuary as an extremely remote consequence of the training-works; whereas by the time the training-walls reached Tancarville and La Roque, the accretions had been so rapid and considerable that the walls were kept low beyond these points, and the works were finally stopped in 1870 for fear of endangering the approaches to Havre, the second port of France, by the silting-up of the estuary. A survey made by Mr. Estignard in 1875, showed that a loss of 272,000,000 cubic yards of capacity at high-water had occurred in the estuary, in spite of the increased volume within the limits of the trained channel, since the commencement of the works, an amount equivalent to nearly one-third of the total capacity of the estuary. Such a result excited so much anxiety about the maintenance of the estuary that another survey was made by Mr. Germain in 1880, from which it appeared that the capacity of the estuary had further decreased by 40,000,000 cubic yards in the five years from 1875 to 1880 (Appendix III.), making a total reduction of 312,000,000 cubic yards in the capacity of the estuary since the commencement of the works.[2] This enormous accretion has enabled large tracts of land to be reclaimed, and altogether about 25,000 acres have been raised above high-water mark; some portions are only partially covered by very high tides, and the areas lying above Tancarville and La Roque have been completely reclaimed (Plate 4, Fig. 1). The value of these lands when fully reclaimed is estimated at about £1,600,000, thus amounting to more than the expenditure on the works; but the Government have admitted the right of the riparian landowners to a share in these reclamations. The lands thus raised

[1] Minutes of Proceedings Inst. C.E. vol. lxx. p. 21.

[2] The accretion measured in the outer estuary, between 1875 and 1880, mainly occurred in deep water, which does not affect the tidal capacity; but it equally diminished the capacity of the estuary, and caused a very important alteration by greatly reducing the depth in the main outlet channel.

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extend as far as Honfleur on the southern shore, and 8½ miles beyond the ends of the training-walls on the north, which shows that the accreting influence of these walls extends far beyond their extremities.

The prolongation of the training-walls has naturally formed the subject of various designs; for whereas Rouen needs a stable and deeper entrance channel, it is essential that the training-works should both supply a channel for Honfleur on the south, and at the same time not divert the ebb and flow of the tide from the entrance to Havre on the north, either by deviating the deep-water channel towards the south, or by greatly restricting the estuary. The principal schemes are shown in Plate 4, Fig. 9; and the most important and latest of these is that of Mr. Lavoinne, propounded in his book on "La Seine Maritime" published in 1885 after his lamented decease. Having charge as Engineer-in-Chief of the tidal Seine from Rouen to the sea, and residing at Rouen, he necessarily was much interested in the development of that port by the formation of a satisfactory channel from Berville to the sea, a problem presenting peculiar difficulties, and of the highest interest to engineers. It would be impossible to rest content with the present state of the outlet, where a narrow trained channel emerges abruptly into a wide shallow sandy estuary, resulting in a barred shifting channel beyond the ends of the banks, which is likely still further to deteriorate from accretion in the estuary. The difficulties consist in designing a line for the training-walls which shall serve both Honfleur and Havre, and in placing them close enough together to fix and deepen the channel without unduly reducing the tidal capacity of the estuary. Mr. Bouniceau planned a narrow winding channel passing close along Honfleur, and then directed out to sea with the northern bank turning up to Havre (Plate 4, Fig. 9), or an alternative scheme taking the trained channel across the estuary from Honfleur to Havre. Both schemes would have greatly restricted the estuary; and whilst the first scheme diverted the channel away from Havre, the second necessitated a sharp bend and a badly placed outlet.

Mr. Lemire proposed a narrow channel, like Mr. Bouniceau's, down to Honfleur, but more sinuous, though it expanded beyond to a trumpet-shaped mouth, an improvement in this respect on Mr. Bouniceau's scheme (Plate 4, Fig. 9).

Mr. Vauthier, in his report to the municipality of Rouen in 1881, indicated the plan he advised for dealing with the estuary. It consists of two training-walls diverging to Havre on the north, and to Honfleur on the south, in continuation of the present channel

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which he proposed to widen out up to Quillebeuf, so that the widening out towards the sea might be more gradual (Plate 4, Fig. 9). A detached portion of wall near the Ratier Bank would prolong the southern training-wall to the sea, resembling in this feature Mr. Lemire's design. It has, however, been objected to Mr. Vauthier's scheme that his training-walls, being so far apart, and mainly on banks already considerably raised by accretion, would have little influence on the channel between them.

Mr. Lavoinne adopts a middle course between the previous schemes, for whilst interfering less with the estuary than Mr. Bouniceau or Mr. Lemire, he places his walls much closer together than Mr. Vauthier, though proposing the same modifications of the existing walls between Quillebeuf and Berville (Plate 4, Fig. 9). Before entering upon the consideration of his scheme, Mr. Lavoinne discusses in his book the results of the hydrographical surveys of Mr. Estignard and Mr. Germain, and the modifications in the tides at Havre observed by Mr. Quinette de Rochemont, M. Inst. C.E., and attributed by that engineer to the silting-up of the estuary. The time of high-water at Havre has advanced thirty-eight minutes; and the reverse current, which makes its appearance near Havre some time before high water, has also advanced about thirty minutes. Mr. Lavoinne considers that this change is due to the increase in depth seawards of Havre, and not to the influence of the diminished capacity of the estuary. Whilst, however, it is possible that the increase in depth has somewhat facilitated the approach of the tide to Havre, it is difficult to admit that an advance of the lines of soundings of which the maximum is 400 yards should have effected such a notable advance in the period of the tide, and that the great diminution in the capacity of the estuary has had no influence on the result. It is far more probable that both causes have contributed to the change, but in what proportions it would be very difficult to determine, though the Author thinks that the preponderating influence must be due to the very great alteration in the estuary produced by the training-walls. As, however, Mr. Lavoinne attributes the deepening in front of Havre to the training-works, the question is one rather of scientific interest than of importance with regard to the future of the estuary, for, according both to Mr. Lavoinne's and Mr. de Rochemont's views, an extension of the training-walls would occasion further modifications in the tidal regime.

Mr. Lavoinne's remarks on the variations in depth, deduced from the hydrographical surveys, are of much greater importance in the consideration of the improvement of the estuary-channel. He

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points out that great changes have occurred at the mouth of the estuary in the past as well as recently, and that thirty-two years ago the capacity of the estuary, between the meridians of Honfleur and Cap de la Hève, was actually less than at present, though Mr. Germain's survey indicated a considerable accumulation in the deep-water channels between 1875 and 1880 (Appendix II.). He attributes these changes to the variable action of storms, rather than to the regular ebb and flow of the tide. The Author agrees with these views as regards the outer portion of the estuary, for the increase in depth has occurred at depths where waves would exert far more influence than the tidal flow, and the changes have been so very irregular (Plate 5, Figs. 1 to 4). Moreover, although Mr. Germain discovered that the deep passes had shoaled between 1875 and 1880 (Plate 5, Figs. 3 and 4), he found that there was some increase in capacity in the section above, which would be more subject to the influence of the decrease in tidal-capacity (Appendix III.). Mr. Lavoinne, however, appears to go a step further, and to argue that the great silting-up in the estuary is the result of a period of storms, tending to remove the sands from outside, and to deposit them within the estuary. Accordingly, Mr. Lavoinne is at issue with Mr. Estignard as to the cause of the deposits in the estuary; for the former attributes them mainly to a succession of peculiar meteorological conditions favouring deposit, which might be succeeded by a series of reverse phenomena as previously, though admitting that the training-walls have somewhat aided the deposits by their shelter; whilst the latter considers the accretion to be wholly due to the training-walls. The Author thinks that the construction of the training-walls may have coincided with a series of peculiarly unfavourable meteorological conditions, which has hastened the accretion in the estuary; but the training-walls, by totally altering the condition of the estuary, have undoubtedly permanently fixed the deposits both behind them and in the sheltered bays in front, so that a recurrence of favourable conditions would no longer, as of old, clear out the estuary. The wanderings of the channels have been restricted, from the ends of the training-walls to a line between Honfleur and La Hoc, by the land which has been raised beyond the influence of the tides, so that the reductions in the estuary are permanent (Plate 5, Figs. 1 and 2; and Plate 4, Figs. 1 and 8).

It is, moreover, probable that the considerable increase in capacity of the outer estuary, which occurred between 1853 and 1875 (see Tables in Appendix II.), may have been due to the sand

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and silt, brought up by the waves and flood-tide, having been deposited behind the training-walls, and having thus been prevented from being carried out again by the ebb-tide, and redeposited in the lower estuary; for this period of erosion in the outer estuary approximately coincides with the period during which the training-walls exercised their greatest influence in promoting the enormous accretions measured by Mr. Estignard. Mr. Lavoinne has overlooked this possibility, and has considered the changes as due entirely to meteorological oscillations, and quite uninfluenced by the convulsion which has occurred in the upper estuary in consequence of the training-works.

Mr. Lavoinne's scheme serves Honfleur by placing the southern concave training-wall close alongside it. The mouth of the estuary is also narrowed by prolonging the southern wall across the existing southern channel, and all along the Ratier Bank (Plate 4, Fig. 9). Besides narrowing the limits of the tidal flow, Mr. Lavoinne anticipates a benefit to the estuary by cutting off the southern channel, up which the tide brings a good deal of silt, whereas the middle and northern channels are tolerably clear; and considers that, by keeping the walls low, the accumulation of deposit behind them will be prevented or delayed. The experience, however, derived from the low banks beyond Tancarville and La Roque affords little hope of such a result.

The Author, whilst acknowledging the great ability displayed by Mr. Lavoinne in his discussion of the subject, and in the scheme he has advised, and whilst agreeing with some of the points urged in favour of the scheme, nevertheless considers that there are important objections to some parts of the proposal. He has already, in his Paper on "Harbours and Estuaries,"[1] expressed full concurrence with the proposed widening out of the existing training-walls, first propounded by Mr. Vauthier; and he adheres to the view that it would be very advantageous if such widening could be extended still higher up, so that the tide might be more freely admitted, and that the widening out towards the mouth of the estuary might be more gradual. He considers, however, that the continuously concave southern training-wall, whilst very favourable to Honfleur, will unduly keep the ebb-current to that side, and therefore away from Havre. Also he thinks that the extension of the wall along the Ratier Bank will act like a groyne, and, arresting the silt-bearing southern current, will connect Trouville Bank with the shore, and lead to a large accumulation

[1] Minutes of Proceedings Inst. C.E. vol. lxx., p. 23.

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of deposit in front of Trouville. Moreover, he is of opinion that the exposed position of the wall on shifting sand-banks will render its maintenance very difficult till deposit has accumulated behind it, and also that the low walls proposed will not prevent accretion.

The channel just below the training-walls is influenced by the ebbing current issuing from the trained channel till it gradually loses its effect; the outer channels are formed by the flood-tide, and modified by the action of storms. Between these channels, there is a zone in the estuary where the flood and the ebb have varying influences, resulting in a continually shifting channel (Plate 5, Fig. 2; and Plate 4, Figs. 1 and 8). In order to improve this central portion of channel, it is necessary to make the ebb act more directly upon it, and as far as possible to concentrate the flood in the same direction. This necessitates the restriction of this portion of the estuary by training-walls, and the Author has given in Plate 4, Fig. 8, the direction for the walls which he believes would accomplish the desired result. He has retained the enlargement in width of the existing trained channel, as advocated by Mr. Vauthier and Mr. Lavoinne, but with the proviso that he would much wish to extend it further up if it was possible. He has dispensed with the continuous curve by narrowing the width slightly more than Mr. Lavoinne at the change of curvature, and by forming a short channel of access to Honfleur, which could be very easily maintained by dredging. This will enable the channel to be carried more directly towards the position of the central deep, and more over towards Havre. He has only placed a regulating wall beyond Honfleur, as he considers that the flood should be freely admitted by all three channels, and concentrated by the funnel-shaped mouth towards Honfleur, where the ebb would be losing its influence. The southern training-wall would not be prejudicial to Trouville; and the northern training-wall would be sheltered from north-westerly storms, and would be soon protected by accretion at the back. The tidal capacity of the estuary will undoubtedly be still further reduced; but by prolonging the influence of the ebb, which is powerfully reinforced by the large discharge of clear fresh water from the Seine, and by bringing the trained channel down to the place where deep water is maintained by the flood-tide and the action of waves, it may reasonably be anticipated that a stable and adequately deep channel will be secured, which will provide a proper access to Rouen without injury either to Havre or to Honfleur.

The Tancarville Canal is in course of construction for the purpose of enabling small vessels to pass between Havre and the

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trained channel without encountering the dangers of the exposed estuary (Plate 4, Fig. 1). Formerly a channel existed along the northern shore, under the shelter of Hoc Point, by which small vessels could reach the estuary above; but the site of this channel has been converted into land by the accretions. The canal extends from the docks at Havre to the trained channel at Tancarville; and the locks and other works of construction on it are being made large enough to admit of the passage of sea-going vessels from Havre to the river at Tancarville, by merely excavating an enlarged section for the canal in the event of such a deterioration of the estuary as to bar the river channel. The Author, however, thinks that the necessity for a ship-canal between Havre and Tancarville may be obviated by judiciously-designed training-works, aided by the large freshwater discharge of the Seine, and the deep water outside.

Concluding Remarks.

The Author, in conclusion, must acknowledge his obligations to Mr. Schwebelé, the Librarian of the École des Ponts et Chausées at Paris, for aiding his inquiries; to Mr. Boulé for information about Suresnes weir, and the cost of the Upper Seine works; to Mr. Lavollée for a section of the Upper Seine and other particulars; and to Mr. Caméré for several drawings and details of the new weirs on the Lower Seine.

The Author hopes that the details he has given about the hydrology of the Seine basin may lead to an extension of hydrological investigations in the river-basins of England and her colonies, and to the adoption of the system of giving warnings of floods.

Though there may be no scope in Great Britain for works equal in magnitude to the new weirs on the Lower Seine, he trusts that the descriptions of these novel types of weirs, whilst necessarily of general engineering interest as marking a progress in this branch of science, may be of special value to engineers engaged in river-works in the Colonies, in India, and in America.

The experience to be gathered from the training-works in the Seine estuary is, in his opinion, of the highest importance, for he believes that, had the results of terminating training-walls in the middle of a sandy estuary been fully appreciated, the cost of two abortive attempts to obtain authority to carry out similar works in the estuary of the Mersey, far less favoured by nature

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than the Seine, might have been saved. For the Seine works demonstrate that accretion is the result of training-walls, whether high or low, in a shifting sandy estuary, and that a deep channel can only be secured by prolonging the training-walls to deep water. The Ribble is an estuary similar in many respects to the Seine, but far less favourably situated as regards freshwater flow and depth at its mouth. Its history also shows that accretion occurs with low training-walls, and that training-works cannot stop in the middle of a wide sandy estuary. The modifications needed in the width between the training-walls of the Seine will serve as a useful warning to prevent the tidal flow of the Ribble from being similarly curtailed.

The communication is illustrated by a series of tracings, to a small scale, from which Plates 1, 2, 3 and 4 have been engraved.

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APPENDICES.

APPENDIX I.

Changes of the Navigable Channel in the Seine Estuary.

Mr. Vauthier, in his Report on the Estuary of the Seine, gives a plan showing some of the changes in the navigable channel between 1874 and 1880: Mr. Lavoinne, in his "Seine Maritime" (p. 160) furnishes a plan indicating changes between 1879 and 1882; and Mr. Audra, in a plan in "Le Génie Civil" (October 1885, p. 411), brings the series down from 1879 to June 1885. The lines representing the channels at the various dates appear compressed into a compact bundle for a little distance from the ends of the training-walls, and after divergencies below, extending right across the estuary, they reassemble again between the Amfard and Ratier banks, a little to the east of the meridian of Havre. The greatest divergence in the earlier series occurs eastwards of Honfleur, about half-way between Hoc Point and the ends of the training-walls; whereas, in the later series, the divergence is more marked to the west of Honfleur, due south of Hoc Point. The transition of the channel across the estuary is sometimes very rapid, the extremes of the series 1874–1880 having occurred between May and December 1878. These changes materially affect the access to Honfleur; for, whereas in 1878, 1879, and February 1880, the main channel passed close to Honfleur, in March 1880 it had shifted to the centre of the estuary; in 1881 it approached Hoc Point, and in June 1885 it ran close along the point, at the furthest possible distance from Honfleur. In considering these changes, however, it must be remembered that the lines only indicate the best out of two or three channels that may be simultaneously in existence, and therefore that a change in the position of the lines does not necessarily imply a complete transposition of the channel, but may result from the gradual improvement of one channel, and the deterioration of another which is thus superseded.

APPENDIX II.

Alterations in the Outer Estuary of the Seine to the West of Honfleur.

The alterations which have taken place in the outer estuary, between the meridians of Honfleur and Cap de la Hève, between 1834 and 1883, as gathered from the hydrographical surveys, are illustrated in a tabular form by Mr. Lavoinne in his book ("La Seine Maritime et son Estuaire," p. 151), which enables modifications in capacity at the different periods to be readily appreciated. The estuary is divided into two portions in both the Tables given; in the first Table, the portions north and south of the Amfard Bank are separately considered;

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and in the second Table, the portions east and west of the Amfard meridian are contrasted. The Tables, converted into English measures, are as follows:—

Periods.	Years.	Alterations in the Banks.					
		North of Amfard.		South of Amfard.		Total.	
		Accretion.	Erosion.	Accretion.	Erosion.	Accretion.	Erosion.
	No.	Cubic yards.	Cubic yards.	Cubic yards.	Cubic yards.	Cubic yards.	Cubic yards.
1834–1853	19	14,841,300	..	51,480,200	..	66,321,500	..
1853–1863	10	54,059,100	..	..	75,805,000	..	21,245,900
1863–1875	12	..	10,141,400	..	37,011,200	..	47,152,600
1875–1880	5	5,575,200	..	50,050,900	..	55,626,100	..
1880–1883	3	11,905,000	..	..	9,986,900	1,918,100	..
Total result 1834–1883		Accretion 76,239,200				55,467,200	
		Erosion		20,772,000			

Periods.	Years.	Alterations in the Banks.					
		West of Amfard.		East of Amfard.		Total.	
		Accretion.	Erosion.	Accretion.	Erosion.	Accretion.	Erosion.
	No.	Cubic yards.	Cubic yards.	Cubic yards.	Cubic yards.	Cubic yards.	Cubic yards.
1834–1853	19	59,932,100	..	6,389,400	..	66,321,500	..
1853–1863	10	..	22,448,400	1,202,500	..	..	21,245,900
1863–1875	12	..	39,958,500	..	7,194,100	.	47,152,600
1875–1880	5	33,238,400	..	22,387,700	..	55,626,100	..
1880–1883	3	516,800	..	1,401,300	..	1,918,100	..
Total result 1834–1883		Accretion 31,280,400		24,186,800		55,467,200	

Mr. Vauthier has drawn out a somewhat more elaborate Table in "Le Génie Civil" (September 19, 1885, p. 331), showing the changes in the outer estuary during the same period of forty-nine years, including in addition the results of surveys taken in 1866 and 1869. His Table refers to the portion of the estuary west of Honfleur, excluding the part to the north-east of Hoc Point, and extending 6,400 feet west of Cap de la Hève. This area is separated into eight divisions; and the capacity of the estuary below the zero of the marine charts (lowest low-water), less the volume of any banks rising above that level, is given for each division at the dates of the several surveys, together with the yearly volume of accretion or erosion in each division in the intervals between the successive surveys. A summary of the total results furnished by the Table for the whole area under consideration is given below:—

1834.	1853.	1863.	1866.	1869.	1875.	1880.
Cubic yards.	Cubic yards.	Cubic yards.	Cubic yards.	Cubic yards.	Cubic yards.	Cubic yards.
433,348,000	387,423,000	414,604,000	437,756,000	490,168,000	486,768,000	441,628,000
Accretion	45,925,000			3,400,000	45,140,000	
Erosion		27,181,000	23,152,000	52,412,000		

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The first line in the above Table gives the capacity of the estuary below the zero at the date of each survey; and the second and third lines give the total amount of accretion or erosion, respectively, which has occurred in the interval, as indicated by the change in capacity.

Both Mr. Lavoinne and Mr. Vauthier agree in deducing from their Tables that the outer estuary is in a state of continual change; that the alterations in capacity by successive accretions and erosions result from variations in the influence of the natural phenomena producing them, and that these changes have no apparent connection with the great alterations which have taken place in the upper estuary.

The large modifications which may occur in the rate of change within a short interval are distinctly indicated by comparing the results of the surveys of 1880 and 1883 in the last two periods of Mr. Lavoinne's Tables, and show the very unstable nature of the outer estuary. Nevertheless it is very remarkable that the only periods during which erosion has been observed hitherto, in the lower estuary, coincide with the construction of the training-walls in the upper estuary; whereas before their commencement, and since their completion, accretion has greatly predominated, and has been more rapid in the latter period than in the former.

APPENDIX III.

Changes in the Seine Estuary between 1875 and 1880.

The following extract from Mr. Germain's report of 1881 shows the changes which took place in the estuary between Mr. Estignard's survey of 1875 and Mr. Germain's survey of 1880; the area of the estuary below the training-walls being divided into a series of bands, running north and south, each 1,000 toises (6,395 feet) in width.

"To sum up, starting from the meridian of the Rille lights, and proceeding westwards, we find, in the Bay of the Seine, between that meridian and one 9,000 toises east of the Hève lighthouse—[1]

Toises.	1875–1880. Cubic Yards.
Rille lights to 9,000 E. (Fiquefleur)	1,951,300 accretion.
„ „ 8,000 E. (Vallée d'Oirdelle) . . .	8,322,700 erosion.
„ „ 7,000 E. (Gainville)	10,215,400 erosion.
„ „ 6,000 E. (Honfleur)	11,014,500 erosion.
„ „ 5,000 E. (Harfleur)	10,349,100 erosion.
„ „ 4,000 E. (Penne de Pic)	3,438,500 erosion.
„ „ 3,000 E. (Fort des Neiges)	7,234,500 accretion.
„ „ 2,000 E. (Eure Dock)	22,224,000 accretion.
„ „ 1,000 E. (St. Vincent du Havre) . .	40,192,200 accretion.
„ „ 0 (Hève Lighthouse) . . .	44,611,600 accretion.
„ „ 1,000 W. (St. Christophe)	40,244,100 accretion.

Accordingly the volume of accretion, between 1875 and 1880, amounts to over 40 million cubic yards, deposited between the meridian of the extremity of the

[1] This first division has a width of about 2,200 toises.

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Seine training-walls, and a meridian about 1¼ mile west of La Hève (3 miles west of the entrance to the port of Havre) which appears to be the limit seawards of the modifications in depth of the Bay of the Seine."

"Moreover it should be noted that almost the whole of these accretions (over 32 million cubic yards) have been deposited between the meridians of La Hève and Fort des Neiges, in the channel between the Amfard and Ratier banks, which has diminished in depth from 59 to 36 feet without any increase in depth occurring in the lateral channels."

Taking the divisions separately, instead of their sum, it appears from the above Table that accretion occurred in the wide division next the training-walls; then erosion in the next three divisions, followed by accretion in all the other divisions except the last.

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Discussion.

Sir Frederick Bramwell.

Sir FREDERICK BRAMWELL, President, observed that the Author had already enriched the Minutes of Proceedings of the Institution by Papers, to which reference had been made during the reading of the present one, and there were probably but few communications that could occupy the members with more advantage than one giving a thorough view of so important a river as the Seine. The Paper had dealt with the system of weirs adopted upon that river, and with the method of giving notice of floods, and of thereby being in a position to be prepared for them, thus preventing their disastrous effects. Lastly, it dealt with works carried out in the lower part of the Seine; works that had been begun within the memory of many of the members then present, and the results of the execution of which had falsified the predictions of the original authors, acting as warnings or instructions to those who had to deal with similar cases in other rivers. As the Author had stated, there were no rivers in England of the magnitude of the Seine, but there were rivers of extreme value, having regard to the commerce upon them and to the interests they served. He trusted the members would discuss the Paper with that thoroughness which was its due.

Mr. Vernon-Harcourt.

Mr. L. F. VERNON-HARCOURT said that Mr. Caméré, one of the French engineers on the Seine, was sending him some photographs of the weirs. He thought that every one who had to do with rivers in England and France must have been struck with the great importance given in France to the system of inland navigation, and with its great neglect in England. There were, no doubt, some cases in which estuaries had been improved; but he was referring especially to the improvement of the inland portion of rivers, and to canals. In France, every year something was done to improve the depth and capabilities of canals and rivers, and most of the works described in the Paper had been executed within the last twenty-five years, so that the advance of railways in France had not at all stopped inland-navigation works, as it had to a great extent in England. The chief advantage that France had was that the inland-navigation works were under the control of the Government, and that their expenses were paid out of the taxes. Whether such a system could be introduced in England he could not say; but he thought it would be impossible thoroughly to improve the system of inland communication with-

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out some general comprehensive administration of the canals, and of the rivers connected with them. Englishmen might not desire to have a paternal Government like that in France; but he wished to remind the members that there was another democratic country, the United States of America, in which the same system prevailed, the improvements in the rivers, and some of the canals, being executed at the expense of the nation. At the present time, if the Government desired to utilize the services of the unemployed, he thought they could not do better than set them to work upon the construction of inland-navigation works. With reference to training-works, he thought that in early days even Mr. Bouniceau was not aware of what such works would produce. In one of his schemes, Mr. Bouniceau had proposed to form a narrow trained channel up to Havre. He felt sure that Mr. Bouniceau had not anticipated that he was training the high-water channel of the river, but thought that he was training a low-water channel. If it was possible for engineers by training-works to fix and improve the low-water channel of the river, and at the same time to admit the full volume of tidal water up the river as before, then training-walls would be most excellent things, and very much better even than they now were; but unfortunately, when a river was regulated by training-walls, especially when the tidal flow brought up silt and sand, then in dealing with training-walls, not merely the low-water channel, but the high-water channel also had to be considered. He might mention the case of the Ribble estuary, which in many respects was similar to the Seine. Fig. 1 showed the training-walls that had been carried out in the Ribble. As in the case of the Seine, they had produced accretion. There, as in the Seine, the estuary was funnel-shaped, and it had been found that the training-walls were not carried far enough, and it had been necessary to get authority to extend them farther, as shown by dotted lines on the plan. Unfortunately, unlike the Seine, the Ribble had a very small basin, about 800 square miles in area instead of 30,000, so that much less fresh water was discharged. Moreover, the training-walls, as authorized, would not be prolonged far enough to reach deep water; and it would be necessary at some future time to apply for a prolongation, as in the case of the Seine. In the proposals for improving the Seine outlet, the training-walls were widened out considerably; whereas it would be noticed that in the Ribble the training-walls were taken to one side, and that very little widening-out was proposed. The result, he believed, would be like that which he feared would occur in the case of Mr.

Mr. Vernon-Harcourt.

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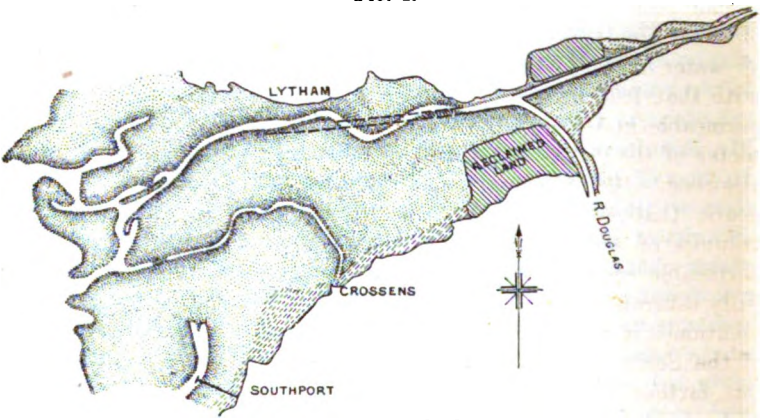
Mr. Vernon-Harcourt.

Lavoinne's scheme as regarded Trouville, but in a greater degree, that the neighbouring seaside place of Southport would have in time sandbanks instead of sea in front of it. If, therefore, training-walls were once begun, it was absolutely necessary to look forward to carrying them into deep water, and to consider that the accre-

FIG. 1.



RIBBLE ESTUARY. Scale $\frac{1}{25000}$.

tions would not only be formed behind, but also along the sides of the estuary in front of the training-walls. It was, accordingly, most desirable that great pains should be taken, when training-works were begun, to consider to what extent, and in what directions they might have to be eventually carried out.

Mr. Baldwin Latham.

Mr. BALDWIN LATHAM observed that the Author had taken immense pains to compress a great amount of reliable information into a very small compass, and his labours would be very useful to those who had to study the question of rivers. There were, however, one or two points on which he should have been glad if the Author had given a little more information. It would have been convenient if he had divided the impermeable areas into the different classes of strata which contributed their water-supply. He had given some valuable information as to the relative quantities of water flowing off from diversified strata, such as granite and lias, but he should be glad to know what the Author really meant by impermeable strata, because there was a great difference as to the quantity of water likely to be carried off from equal areas of different impermeable strata. It was, no doubt, common to classify together all clay soils and all rocks of an impermeable character, but there was an immense difference in the character of

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the two classes. It might be said that granite, slate, gneiss, and rocks of that character, were absolutely impermeable, as they allowed no water to pass into them; but lias and clays of various descriptions were impermeable, from the fact that they allowed the largest percentage of water to be absorbed by them; so that two qualities diametrically opposite produced impermeable strata. It had been ascertained, by constant gaugings, that the quantity of water absorbed by clay areas was enormous, and that the volume of water flowing from them was very small as compared both with that from areas of a permeable character and also from impermeable areas, where the impermeability was due to the character of the rocks, which would not permit of any water being absorbed. He had ascertained by gaugings during the last ten years, that most of the permeable soils yielded much larger volumes of water to rivers than those that were classed as impermeable strata. He did not allude to clay soils, which when fully saturated caused the water to flow from the surface. It was mentioned in the Paper that a very large area of sand in the basin of the Seine did not contribute any water whatever to the river; but farther on the Author stated that it did probably furnish water to springs in the lower part of the river. It had been ascertained from observations made by the late Mr. Charles Greaves, M. Inst. C.E., upon the question of percolation,[1] that a much larger quantity of water absolutely passed into and through sand-gauges, than into those of any other description of strata, and he was satisfied that upon a closer investigation it would appear that those areas, instead of yielding little or no water to the river Seine, yielded a very large percentage. In another case, the quantity derived from permeable strata was only given as $\frac{1}{7}$ of the total rainfall. It was interesting to notice that the total results of the gauging of the Seine, agreed closely with those of the gaugings of the Thames. The absolute quantities flowing off, as measured in depth, were almost identical during a long series of years. No doubt in the higher districts of the Seine, the rainfall was heavier than in the higher parts of the Thames, but the average quantity of rain was almost identical upon the whole area of the river, and there was a similar agreement with regard to the quantity flowing away. The diminished fall of rain about Paris, which was not quite in the centre of the basin, might, he thought, be readily accounted for. It would naturally occur in such a position, taking into consideration the meteorological conditions.

Mr. Baldwin Latham.

[1] Minutes of Proceedings Inst. C.E. vol. xlv. p. 19.

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Mr. Baldwin Latham.

It would be observed in the first place that the diminished fall occurred over an area of a permeable character. The air over such an area would be naturally drier than in districts where the soil was of an impermeable character. Then it would be noted that farther down the river, the rainfall in the lower districts increased. This was partly due to the flow of the river being in a north-westerly direction. It should also be borne in mind that rain mostly progressed from the west; and the vapours to the west were more laden and in a better condition to drop moisture than those in the east. The consequence was, that owing to the high lands to the west of Paris, the rain would be dropped there before the clouds reached Paris, while the vapours at a loftier elevation passed on to higher grounds. Rainfall was also increased by diminution of temperature, and as the mouth of the Seine lay a considerable distance north of Paris, this was a condition favourable to an increase of rainfall on what appeared to be the moist part of the drainage area. It had been stated in the Paper that the most ancient floods had been the highest. Having recently had occasion to collect records of floods in England, he had found that the result agreed with that stated by the Author; but he did not know whether it was absolutely due to river improvements and the more rapid carrying away of floods, or whether (as might possibly be the case) there were formerly much greater differences between the extremes of drought and flood, than at the present day. With reference to the remarkable flood in the Seine and in other places in 1880, he might mention that in the year 1879 there had been, as far as rainfall was concerned, winter in the summer. It was the most disastrous year ever known in the agricultural annals of the country; and at the end the springs were very high. In the winter of 1879–80, however, there was the smallest rainfall for the season recorded in the South of England for many years, and if that had occurred with the ordinary condition of the springs there would have been a very severe drought. With so much water stored in the ground during the year any little rainfall would take effect. There was no difficulty whatever in predicting floods, especially in a river like the Seine, seeing that the floods always descended from the highest to the lowest districts; but, in rivers of less length than the Seine, the warning that could be given would of course be shorter. In certain conditions of springs and streams predictions might be made with considerable accuracy for long periods beforehand. During the last ten years he had been able to foretell the appearance of one of those remarkable streams in the

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neighbourhood of Croydon, called bourne-flows, which would be found mentioned by many old English writers as presaging war, pestilence, or famine. He had been enabled regularly to predict the day when the spring would break out, and the volume of the water. He would now venture to predict that about the 17th of March a small bourne would break out in Caterham Valley a few hundred yards below the "Rose and Crown."[1] These bourne-flows were produced by a wave of water which passed through the ground from the highest to the lowest part of the district. In a year like the present the difficulty of forecasting became extremely great, owing to the fact that the springs were already rapidly falling in the upper portion of the district, in fact much more rapidly than they were rising in other portions of it. Mr. Baldwin Latham.

Mr. W. SHELFORD remarked that the Seine had been for many years represented as a river which had been improved by large works, which works, however, had resulted in a deterioration of the estuary. It was satisfactory, therefore, to have a Paper setting out the facts of the case very fully. Taking a broad view of the general result, it amounted to this, that the channels of the Seine had increased in depth considerably, and that that increase in depth had been accompanied by a diminution in width and a considerable reduction in tidal-capacity. But that was not an unusual thing. Nature had brought about the same result in the Humber, on the outfall of which he had submitted a Paper many years ago to the Institution.[2] The channels in the Humber were now narrower and deeper than ever, and that narrowing and deepening had been accompanied by a considerable reduction in tidal-capacity. On the Thames the same thing had been brought about by nature and art combined, and on the Clyde art had accomplished a similar result. In fact, it was not, he thought, too much to say that in the case of all tidal-rivers where there was an increased depth in the navigable channel, it was accompanied by a diminution of width, and as a rule by a reduction of the tidal-capacity. It was stated in the Paper that from the ends of the training-walls at Berville down to deep water in the meridian of Havre, a distance of about 13 miles, there had been no alteration in the channel. He gathered that it was not silted up, that it was not scoured, and that it was still a shifting channel, which was of course very incon- Mr. Shelford.

[1] This bourne rose in holes in the bourne ditch on the 19th, and continued to the 26th of March, after which it entirely disappeared.

[2] Minutes of Proceedings Inst. C.E. vol. xxviii. p. 472.

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Mr. Shelford. venient, and even somewhat dangerous. In those facts he thought there was nothing to lead to the inference that the works of the Seine had led to its deterioration. It appeared to him that if those works were continued judiciously to the sea, the Seine would be an example of one of the most complete successes of engineering works of that character, notwithstanding a very large reduction in tidal-capacity. That reduction amounted to 312,000,000 cubic yards, equivalent to about one-third of the whole tidal-capacity of the estuary, or about one-half the tidal-capacity of the Mersey. It would be found, on reference to the Author's Paper on "Harbours and Estuaries,"[1] that Mr. Bouniceau had anticipated that storms, waves, and flood-tides would bring up the sands of the estuary, and deposit them in course of time behind the training-walls, and that had actually happened, but in a much shorter time than had been expected. The Author had exhibited a diagram showing the simultaneous tidal curves of flood- and of ebb-tides on the Seine (Plate 4, Fig. 11). It showed the fall of the surface of the water at various times. If the same channel, with the same depth of water in it, had a certain fall, the velocity in that channel, whether at flood- or ebb-tide, would depend entirely on the fall, and it was consequently manifest that, as the fall was much greater on the flood-tide, especially in the earlier portions, than on the ebb-tide, the velocity was greater on the flood than on the ebb, and if the velocity was greater the scouring power must be greater. There was evidently in that estuary a strong natural tendency on the part of the tidal-action to scour material from the bottom, and to transfer it to the upper reaches of the estuary, from which it could only be brought down by great floods of fresh-water, rather than by the ebb-tide. When he saw that condition of things (which was more marked in the Seine than in most rivers) he was not at all astonished to find that there had been so large an accretion.

The Author had stated in his concluding remarks that, "The experience to be gathered from the training-works in the Seine estuary is, in his opinion, of the highest importance, for he believes that, had the results of terminating training-walls in the middle of a sandy estuary been fully appreciated, the cost of two abortive attempts to obtain authority to carry out similar works in the estuary of the Mersey, far less favoured by nature than the Seine, might have been saved." Like many other engineers, Mr. Shelford was engaged in that inquiry, and therefore

[1] Minutes of Proceedings Inst. C.E. vol. lxx. p. 21.

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knew something about the circumstances of the case, and he did not think that the statement in the Paper was quite a fair one. The Author had compared the training-walls of the Seine with those proposed in the Mersey, because they terminated in the middle of a sandy estuary. But what were the facts? In the Seine it was true that they did so; they were 10 miles from deep water; in the Mersey it was proposed to terminate the training-walls at a distance much under a mile from Garston Deep. The Mersey, moreover, had a peculiar tidal-action, due to its form. It was not a river, it was a tidal reservoir, in which the flood-tide entered with great force on the Cheshire side, and normally returned on the ebb on the Lancashire side, that side being 50 per cent. shorter than the other; the ebb-tide, therefore, retired by the shortest route, and that action alone would have the effect of making stable the deep-water channel from those training-walls to Garston Deep. It was scarcely fair to compare one river with another. There were great differences between the Mersey and the Seine. One was that there was a great fretting action going on in the Mersey which did not exist in the Seine. Again, there was a bar at the mouth of the Mersey 10 miles out at sea, and there was no similar bar at the mouth of the Seine. That at the mouth of the Mersey was a disgrace to the port of Liverpool. Not only had the Mersey conservators done nothing to remove it, but he did not believe that they even knew the cause of it. Mr. Shelford.

Mr. VERNON-HARCOURT said that several photographs of movable weirs on the Seine were now on the table. They had been sent to him by Mr. Caméré and Mr. Schwebelé. He had also placed on the table two photographs of an English tilting-weir at Throstle-nest, near Manchester, across the Irwell, which had been given to him by the designer of the weir, Mr. F. Wiswall, Assoc. M. Inst. C.E. Five photographs were specially referred to by Mr. Caméré as particularly worthy of notice. The first showed Port Villez weir as originally constructed, and the second the same weir as modified in order to enable the frames to be laid down with greater safety. Both the photographs showed well the manner in which the frames were laid down in the bed of the river. The other three represented the frames of Poses weir when lifted. They had been only taken last month, and they showed the amount of *débris* collected in lifting the frames, which Mr. Caméré considered would have rendered it impossible to open properly a weir like that at Port Villez, if such a form of weir had been adopted. Mr. Caméré therefore thought that, in such a situation, the only weir that could be adopted would be one like that erected at Poses. The Mr. Vernon-Harcourt.

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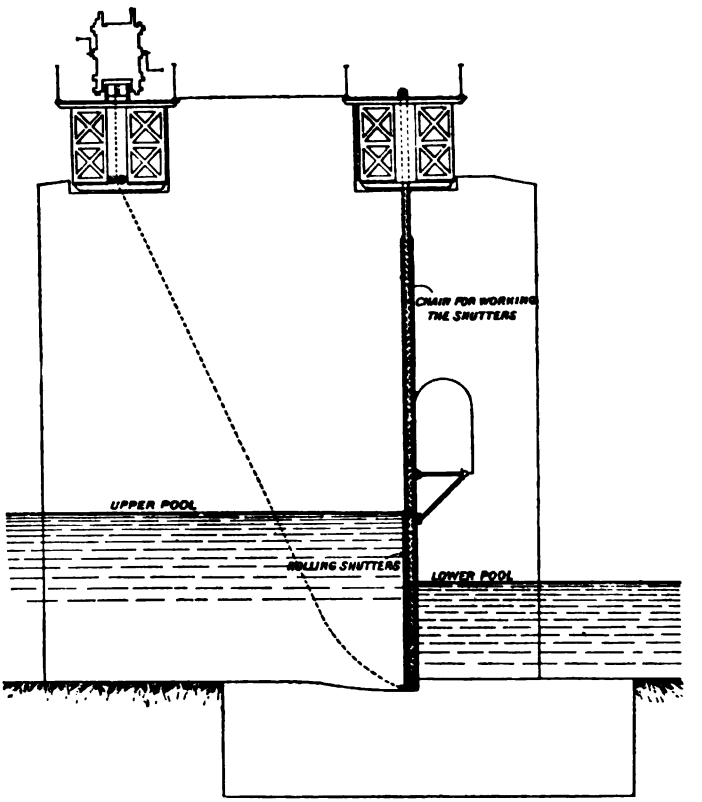
Mr. Vernon-Harcourt.

members, he was sure, would agree with him in thinking that they were much indebted to Mr. Caméré and to Mr. Schwebelé for so kindly acceding to his request and placing the photographs at their disposal. Fig. 2 represented the system of weir designed by Mr. Tavernier in 1873 for the Saône, to which he had referred in the Paper, p. 234.

FIG. 2.



Scale $\frac{1}{125}$.

SYSTEM OF WEIR DESIGNED FOR THE SAÔNE BY MR. TAVERNIER IN 1873.

Mr. Williams.

Mr. E. LEADER WILLIAMS wished to make some remarks, first on the Upper Seine, and secondly on the Estuary portion of the river. He knew no more interesting river for an engineer to visit than the Seine. He had spent many days upon it with advantage, and if the younger members of the profession wished for a pleasant holiday, they might well take one between Havre and Rouen and

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Paris. There was much to learn on the river; the scenery was charming, and, whether in a row-boat or in a small steamer, such as he had himself hired at Rouen, it was possible to get a good view of the works, and to acquire much useful information. The upper part of the river was an ordinarily canalized river, something like the Severn, and still more like the Weaver, the new works of which he had himself designed. The locks on the Seine were of considerable length, some of them being 520 feet long by 39½ feet wide. He thought that English engineers might learn a lesson from the fact that the French engineers did not put the lock-sills down low enough. The original locks had 5¼ feet of water over the sills, but in a comparatively short space of time they had to be deepened to 6½ feet, and the question was now being debated whether they should not be at a still greater depth. Perhaps he had gone to an extreme on the Weaver in putting the sills down at 15 feet. He had adopted the same width, 40 feet, but the length was only 200 feet, because it was much easier to lengthen a lock than to deepen the sills. French weirs had been well discussed in Mr. Vernon-Harcourt's Paper on that subject. He thought—and he was glad that the Author agreed with him—that they were hardly adapted to English rivers. Solid weirs, like those built by Sir William Cubitt, were, he thought, better adapted to English rivers than any other kind; but if movable dams were required, Mr. F. G. M. Stoney's sluices were admirably fitted for the work. They could be lifted clear of the water to afford sufficient headway for the accommodation of the traffic in times of flood. A lesson had been given by French engineers that was learned on the Severn, namely, that it was no use, after getting out of the tideway to the upper reach of a river where the fall was considerable, say 12 or 15 inches in a mile, to think of dredging only. It had been tried on the Seine and on the Severn, and had failed. In an ordinary river there was a succession of pools formed by the shoals below, and if these shoals were dredged, the pools were merely lowered, but the navigation was not improved. It was necessary to substitute for the natural shoals something else, and the French engineers on the Seine, like the English engineers on the Aire and Calder and on the Severn, had properly substituted weirs and locks. It was satisfactory to learn that the trade on the Upper Seine had doubled between 1868 and 1884. A navigation could be made to prosper, only by increasing the size of the vessels which plied on it; the expenditure did not grow in the same proportion. There was the advantage of a permanent way that did not wear out; and by

Mr. Williams.

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Mr. Williams. increasing the sectional-area so as to enable vessels of larger burden to pass with facility, the cost of transit per mile was diminished, and it was possible to compete successfully with railways. With reference to the Lower Seine he had been struck with the remark of the Author at the end of the Paper, to which Mr. Shelford had already referred. He thought that the charge was rather a grave one. He did not take it personally, because he had to thank the Author for assisting the promoters of the Manchester Ship-Canal in the last eventful fight when the victory was won; but reference had been made to "two abortive attempts," and as Mr. Abernethy and he had to accept any odium that might attach to them, it was perhaps natural that he should endeavour to show that the conclusion drawn in the Paper was not justified. He could bear lightly the charge made by the Author, because he sustained it in conjunction with many other engineers. No less than four Past-Presidents of the Institution gave evidence in favour of the training-walls in question, in addition to the Engineers of the Tyne, the Tees, the Clyde, and other rivers. The Author had referred (p. 242) to the condition of the river before the works were carried out, and stated that "shoals rose so high at Aizier and Villequier that the depth of water over them at high-water spring-tides was about 10 feet, so that only vessels of from 100 to 200 tons could go up the river, and frequently had to wait for a high enough tide. Four days at least were occupied in the voyage between the sea and Rouen; and the perils of navigation were aggravated by the bore." That showed the state of things before the estuary-works were carried out—that vessels of a maximum size of 200 tons took four days to get to Rouen. Further (p. 244) the Author stated the result of the works. "The training-works have effected a very remarkable increase in depth between La Mailleraye and Berville, as indicated by comparing the longitudinal sections of the channel in 1824 and 1875 (Plate 4, Fig. 2). The minimum navigation depth between Havre and Rouen, at high-water neap-tides, has been increased from 10 feet up to 18 feet. Vessels of 2,000 tons can navigate the river; and it has been found possible, under favourable conditions, to bring up a vessel drawing 21 feet. Moreover, the charge for freight between Havre and Rouen has been reduced to one-half, and the rate of insurance for Rouen is the same as for Havre. The trade of Rouen has consequently greatly increased, and it has become the fifth port of France." If the result of the works on the Mersey had been to do as much for Manchester, more could hardly be asked for. Remembering the struggle that

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he had been engaged in for the past few years, he began to think whether the Author had looked at Havre as being the Liverpool, and at Rouen as being the Manchester of the Seine. The natural inference from that would be that it would be shown in the Paper that Havre or Honfleur had been damaged; but there was no representation by the Author to that effect. Indeed the very reverse was stated, for there were one or two sentences tending to show that the approach to the port of Havre was rather better now than it was formerly. What principle should be pursued? If a river was designed only for the benefit of some port at its mouth, all sorts of questions might be raised about the equilibrium of the shifting sand-banks and currents; but here there was a positive result, a great benefit to a port inland, and it was certainly a benefit to a country to make such an inland port—the fifth port in France, without damage to any one. The question of accretion was undoubtedly an important one; but still, if accretion occurred, and no damage was done to any existing interest, it was not a very bad thing. The result of accretion in the case of the Seine was remarkably good, for the Author had stated that the land accreted had realized the sum of £1,600,000, which paid for all the expenses of the great river-works that had so benefited Rouen. He thought that any engineer designing those works would naturally expect the accretion that had occurred, as the French engineers had expected it. He admitted that any engineer, on an estuary like the Seine full of silted matter, who raised a training-wall above the level of high-water, and tied that wall into the side-banks in the way that all the Seine training-walls had been tied, made an ordinary and natural trap for the deposit of silt; and on the Dee, where he was engaged in carrying out some works, that result had also followed. Telford, and other engineers who had designed the training-walls on the River Dee, had done so with the avowed object of making accretion, and reclaiming the land; the engineers on the Seine had done the same, and they had achieved their object, and paid for the works out of the land they had made without damage to any one. Another point which the Author had brought out in his Paper (p. 245), was that "The improvement effected in the channel by the training-works has caused a lowering of the low-water at Rouen 2 feet 3½ inches at spring-tides, and 2 feet 9½ inches at neaps; whilst the high-water level has remained unaltered. The tide reaches Rouen fifty-eight minutes earlier than formerly, namely, about two and a quarter hours after high-water at Havre." No better proof could be given of the improvement to the river than that the

Mr. Williams.

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Mr. Williams. works had lowered the low-water level 2 feet 6 inches, on the average, on all tides 74 miles from the sea. As the result of the works, it was estimated by the French engineers, that the scouring of the Seine after the training-walls were completed, had taken out 80,000,000 cubic yards of sand from the upper portion of the river. What did that mean? The estuary of the Seine, measured across from Havre to Honfleur, was about 5 miles in width. The accretion had taken place high on the sand-banks at the mouth. It had displaced tidal-water at a high level which would have ebbed out over a great expanse of open sea 5 miles in width. It would puzzle any engineer to determine what the diminution of the velocity of the current would be across that 5 miles caused by accretion; but any engineer could understand the gain of a drop of 2 feet 6 inches in the low-water level 74 miles above. With water going down late in the tide through the low-water channel, the gain was obvious, and that was a gain similar to what had occurred in the case of the Seine.

He next came to the second portion of the question how far the case of the Seine was parallel to that of the Mersey. Even supposing (which the Author had not proved) that the Seine works had been in any way an injury, it did not follow that the Seine and the Mersey were on all fours with each other. In regard to configuration, there were in the Mersey two estuaries, the outer estuary, which would compare with the Seine up to Quillebeuf; then came the "narrows" between Liverpool and Birkenhead, and then the second estuary. In the Manchester Ship-Canal inquiry, the opponents of the scheme always said that it was impossible to compare the Clyde, or the Tyne, or the Tees with the Mersey, which was a peculiar river, and they now adduced the case of the Seine. In fact, the Seine and the Mersey, in regard to configuration, were in no ways comparable, being utterly distinct. If it had been proposed to train the outer portion of the Mersey between the Rock Lighthouse and the bar there might be something in it. But how was it proposed to treat the Mersey? The Upper Estuary would be a good navigation if the low-water channel could be kept in a given line, but it wandered about at random. It had been part of his duty to buoy and light the Upper Estuary for a long period, and a steamer and crew had been kept constantly going about shifting the buoys day and night. When he stated that the navigable channel once shifted 3 miles across the estuary in twelve months the members would understand the present state of things. All that it was proposed to do in the upper portion of the estuary was to confine the forces of

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nature in one given line. It could not be beneficial that the upper channel should be constantly shifting and sending down material to diminish the depth on the bar. It was not proposed to carry out the works to the high-water level; it was not proposed to carry works to what were called half-tide walls; the height at which the walls were fixed in the Bill was only to the level of the existing sand-banks. That being the case the advocates of the scheme merely retained the low-water channel in one given spot, and prevented what was called fretting. If accretion was to occur with that state of things they must have prevented the powers acting which kept down accretion. What were those powers? No one would deny the effect of currents, but as the training-walls were only to go to the level of the sand-banks there was no interference with the effect of currents passing over them, and as there were sand-banks in the Mersey, particularly one at Eastham, through which the low-water channel had not gone for many years, and that bank had not increased above its ordinary level, it was evident that the natural powers there were sufficient to keep it down to that level. But the question of the power of wind and waves ought not to be lost sight of. The bar at Liverpool was an impediment which varied year by year. The depth at low-water spring-tides had been only 7 feet and at other times 17 feet. What was the power that caused that variable state of things? If there had been 17 feet over the bar in 1885 and it had gone down to 7 feet in 1890, that might have been regarded as the effect of the Manchester Ship-Canal training-walls if they had been constructed meanwhile. But the Upper Mersey estuary was now left in all its integrity, and yet the bar was shifting and changing. It only showed what Sir John Coode had repeatedly pointed out, that the wind and the waves were the greatest powers in a wide open estuary like the mouth of the Mersey or the mouth of the Seine. The Author had given an interesting Table (Appendix II.), showing the capacity in cubic yards of the portion of the estuary west of Honfleur, corresponding very much with the position of the Liverpool bar, and it would be found that in 1834, long before the works were commenced (in 1853) the capacity was 433,348,000 cubic yards, and in 1869 it was 490,168,000 cubic yards, an increase of nearly sixty million cubic yards. In 1875 it fell to 486,768,000, and in 1880 to 441,628,000. Thus long after the works had been completed, the tidal cubical contents of that portion of the estuary were nine or ten million cubic yards more than in 1834. It was impossible, therefore, from those figures, to draw the conclusion that the works

Mr. Williams.

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Mr. Williams. were the sole cause of the changes outside. It was clearly also a question of winds and waves. It had been stated that the training-walls ought to have been carried out to deep water. There was deep water at Garston, and below Garston there were the narrows. Surely the Author could not have meant that the training-walls should be carried through the narrows, and so on to the sea. They were carried on to a point where large vessels could get to Garston, which was deep water; so that one condition laid down by the Author had been fulfilled. The Liverpool bar had, of course, been a great question in the consideration of the Manchester Ship-Canal. There was virtually no bar on the Seine. Again there was a bore of a formidable character in the Seine, but there was none in the Mersey. He thought it was rather a disgrace to Liverpool that the question of the improvement of the bar had not been dealt with. An eminent engineer, Captain Eads, had been brought from America, who had laid down some good elementary principles on hydraulic engineering, and if Liverpool had kept him for a short time to remove the bar, as he had in the case of the Mississippi, some benefit might have been done to the river. He had been very much struck with some evidence that had been given by Mr. Thomas Stevenson, M. Inst. C.E. He was asked how many rivers he had improved, and he mentioned a large number. He had carried out training-walls, he said, in many of them, and accretion had followed; but it had not interfered with the improvement of the river, or caused shoaling at the bar in any single instance. It was impossible to put the case stronger than that. In conclusion he desired to say that having had occasion to read much on the subject then under consideration, he had found great benefit in studying the Minutes of Proceedings of the Institution, and especially the remarks of those members who had from time to time occupied the chair. There was one point upon which they had been absolutely consistent and unanimous, and that was in the declaration, that in treating of improvements in rivers they ought not to be dogmatic; because no two rivers were alike. Their profession was too grand to be trammelled in any such way. He hoped that the younger members of the Institution would always bear that in mind. All rivers should be studied, and having brought together all their knowledge from actual experience and reading, they would be better able to lay down correct lines for the improvement of a river or an estuary.

Mr. Abernethy. Mr. J. ABERNETHY, Past President, said that Mr. Leader Williams had ably stated various points to which he had himself intended

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to refer, and his remarks would therefore be of a more general character. The Paper had been well and carefully drawn up, and the description given by the Author of the works for improving the Upper Seine would no doubt afford most valuable information to the younger members of the profession. The works carried on in the Lower Seine, from Rouen to near Havre, were much on the same lines as similar works carried on in rivers in this country and abroad, and with the same satisfactory results. Much, of course, remained to be done. The training-walls should be carried further seaward, so as to fix the channel from Havre to the sea in one given direction. A great deal had been said with regard to accretion, and it had been stated that it had taken place to such an extent, as absolutely to interfere with the channel seaward of Havre, and that the effect of the improvements in the Upper Seine, from Rouen to the end of the training-walls, had been a manifest injury. It was obvious that accretion must take place, from the enormous quantity of material moved from the bed of the river by the confined current between the training-walls, and that accretion was no doubt deposited upon the banks lying between Honfleur and Havre; in other words, a certain portion of the shallow tidal stream, which ebbed and flowed over those banks, was arrested. And what was the tendency of that? Undoubtedly, to throw a greater volume of water into the proper navigable channel. He knew the Seine forty years ago, when small sloops and barges were the only class of vessels that could reach Rouen, and that only in three or four days. On his visit to Rouen the year before last he found vessels of more than 1,000 tons at the quays, and in going down to Havre he passed a vessel of nearly 2,000 tons, drawing 20 feet of water, proceeding to Rouen. There could, therefore, be no doubt as to the success of the operations so far carried out by the French engineers. He had made a point of inquiring as to the effect of the works upon the channel from Havre seawards, and the general information he had obtained was that they had been rather beneficial than otherwise. He thought that was evident from a passage in the Paper, p. 248: "The time of high-water at Havre has advanced thirty-eight minutes; and the reverse current, which makes its appearance near Havre some time before high-water, has also advanced about thirty minutes. Mr. Lavoinne considers that this change is due to the increase in depth seawards of Havre, and not to the influence of the diminished capacity of the estuary." He quite agreed with that statement. He was rather surprised at the

Mr. Abernethy.

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Mr. Abernethy. remark at the end of the Paper with regard to the projected works devised by Mr. Leader Williams and himself in the Mersey. The Author pointed out the great danger of carrying training-walls to the middle of a sandy estuary. That was not the case in the Mersey, the walls were projected through the sandy section to deep water. In the case under consideration although all the works had not been carried out, the result was simply this, that while the navigable channel to Havre had been rather improved, a deep-water communication had been formed with one of the most important cities in France, and a great inland port had been created. Such results reflected the highest credit upon the engineers who designed and carried out the works.

General Rundall. GENERAL RUNDALL, R.E., observed that he was not personally acquainted with the Seine or any of the works described in the Paper, but there was a similarity in that river to some rivers in other countries; and in regard especially to the phenomena of floods, the experience of those who, like himself, had been in India, might perhaps be of interest. In the section on floods, the Author had stated that, "The occurrence of high floods is due to the concentration of the rainfall at special periods of the year, and not to a high average rainfall throughout the year." To a certain extent on the Seine that might be strictly true, but in tropical countries, where the rainfall was confined to a very short period of the year, a high average rainfall did to a certain extent regulate the extraordinary floods which happened. But there was one point which had not been noticed in the Paper, and to which extraordinary and exceptional floods might be due, namely, the configuration of the basin itself. When the catchment-basin of a large river assumed the form of a parallelogram, being long and narrow, if the storm producing the rainfall struck the lower end of such basin and travelled up it, evidently the tributaries at the lower end would run off a greater portion of their volume before the tributaries on the upper part of the river nearer the source were even filled; therefore the flood-volume of the whole basin did not arrive simultaneously at a particular point. In the case of a basin whose length and breadth were more nearly equal, a storm striking the outer part on one side would take some time to travel across to the other, and therefore the floods in those tributaries first affected would commence to contribute their portion, but the distance not being so great that the whole volume could have time to run off, they would arrive at a certain position on the main river, such as Paris on the Seine, more simultaneously. The catchment-basin of the Seine above Paris, which had two

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main tributaries that fed the main river itself, was nearly circular, but suddenly narrowed there, so that if Paris were taken as a centre it resembled a sector of a circle with a radius of 150 or 160 miles. Paris itself was nearly equidistant from the two extremities, and consequently the volume of water which had to run down the tributaries would arrive almost simultaneously at Paris, were it not for other considerations connected with the strata. But supposing a storm to strike the circumference of the Paris basin it would have to travel across to the other side before the whole effect of it would be felt. In the case of cyclones in India it had been found that those storms gyrated on their axes sometimes at the rate of 100 miles an hour, whilst their onward course was at the rate of not more than 10 miles an hour. Consequently if such a storm occurred on the Seine basin it would take fifteen hours to travel across to the other side. If such a thing as a cyclone did occur there, or a very heavy rainfall, he doubted whether the main river could carry the water off at Paris, but fortunately in temperate countries cyclones did not occur, and it was seldom that there was a very heavy fall of rain simultaneously over such an area as even 31,000 square miles. He might be permitted to refer for the sake of comparison to two rivers in India. The basin of one of them, the Mahanuddy, in the province of Orissa, somewhat resembled that of the Seine, but was on a larger scale. It had a course of about 500 miles from its source to the sea, and a basin of 45,000 square miles. A curious feature about it was that its tributaries partook more or less of the same shape as the whole basin. The floods which occurred on that river were very heavy. The Mahanuddy, after collecting all the waters brought by its tributaries, ran through a long and narrow gorge flanked by high hills, and when it arrived at the end of the gorge, it precipitated the whole of its volume on to the delta, where it was carried off by several effluents. When storms occurred which affected only a portion of its tributaries, the rivers in the delta were able to carry them off, but in a great storm, like that in 1866, which travelled across the basin, the delta rivers were not sufficient to carry off the whole volume, and tremendous devastation occurred from the floods, thousands of square miles being inundated. Of late years effective embankments had been constructed, and a great portion of the country formerly liable to be flooded was now protected. The great rainfall in 1866 occasioned a flood which lasted about ten days, and during that time 22,500 million cubic yards were discharged, equivalent to a rainfall of 6 inches over the whole area. After the tenth day, instead of

General Rundall.

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General Rundall.

the river continuing to fall, it began to rise again, and it rose to within 6 inches of the previous level; the duration, however, was only four and a half days, and the total volume of water discharged during that time was about half that in the former flood. The rain that fell was at the rate of 1 inch per day over the whole basin. It was impossible to tell the exact proportion that flowed off, because it was on such a large scale. That case was almost parallel to the instance cited by the Author, of the Seine flood in 1883, as consequent upon two heavy falls of rain following close upon each other. The other river to which he had referred was the Damooda, the shape of which was nearly a parallelogram, whose length was 220 miles, average width 35 miles, and area 7,200 square miles. In 1859, which was a year of exceptional flood, there was a rainfall of 5·50 inches in one part of the basin, varying to 1·80 inch in another part, in twenty-four hours. The character of the river was torrential; it had a slope of 40 feet in 3½ miles, and that slope gradually decreased to 2½ feet per mile when it reached the gauge which recorded the height and fall of the river. The gauge was about 60 miles above the confluence of the Damooda with the Hooghly. The flood reached that gauge in twenty-four hours. Its total duration was three days, but the maximum height lasted only about three hours. The total volume which passed in the three days was 2,500 million cubic yards, equivalent to a flow of rain of 4·10 inches off the whole area; whilst the total rainfall, as far as it could be ascertained, was about 11·10 inches over the basin. The rain-gauges, however, were too few in number to enable any accurate inferences to be deduced, so that when averages of rainfall were spoken of as regulating floods, it was necessary to be cautious in ascertaining how far they had been obtained from a sufficient number of gauges. As to the prediction of floods by the heights of tributaries feeding the main river, he did not think that that would be practicable in India, because the sources of these large rivers lay in uninhabited forest tracts of a deadly character, so that there was no possibility of ascertaining the height of the tributaries. But on the large rivers, like the Mahanuddy, there was telegraphic communication, so that it was possible to know when the maximum flood was passing certain points, and when it might be expected at the head of the delta, which was the crucial point of the river. The question of wave-propagation was an interesting one, but it had not yet been well worked out. He was surprised to find from the Paper that the speed of the wave of propagation on the Upper Seine was 5·72 miles an hour, as the rate of its travelling from

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the extreme point Clamecy to Mantes, 228 miles, ranged from 2·00 to 2·21 miles per hour, which seemed to correspond to what had been observed in India. The Paper would no doubt be useful to engineers in India, though the works carried out on the Seine were not generally applicable to India, because the sectional-area of the beds of the rivers in dry weather was so very large in proportion to the actual water-section; and works that could be carried out on a river like the Seine—on the Upper Seine and down toward the tidal reaches—could in very few instances be adopted in India. But engineers in India had been glad to take advantage of some of the patterns of weirs that had been executed on the Seine, with certain modifications which local circumstances required. They had built them in the large dams to act as scouring sluices, and they had been found to answer exceedingly well. He looked upon that section of the Paper, which treated of internal navigation, as a most important part of it; for it appeared to him germane to some questions of the present day in England. A great deal had been said about preferential railway rates as bearing so hardly on the trading and commercial community generally, so, as he had said elsewhere, he thought it was a great pity that England did not revert to her old national mode of water-carriage. There was room for both rail- and water-transport, and if England only followed the lead of the Continent in that matter she would act wisely. He had taken great interest in Mr. Leader Williams' project for the Manchester Canal, as in all other canals that had been mooted in England and elsewhere. One of the great features of the day was the National Conference at Brussels last year, which he believed was to be renewed this year at Vienna. If the magnificent projects which had been suggested in that conference should ever be brought about they would revolutionize the question of carriage throughout Europe. General Rundall.

Mr. G. Wells Owen observed that the Author, in Appendix III., with reference to the changes in the Seine Estuary, between 1875 and 1880, had given some statistics of the amount of accretion and erosion that had occurred in different parts of the estuary in that period. For the purpose of taking the quantities, the engineers had divided the river into sections, the first section commencing at the end of the training-walls, at Berville, and extending 2,200 toises, or about 2½ miles down the river, the remaining sections being each 1,000 toises, or 1½ mile in length. It was often said that anything could be proved by statistics, and he wished to make a few observations upon this Appendix with a view of preventing wrong inferences being drawn from the Mr. Owen.

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Mr. Owen. statistics in question. It was stated that in the first section of the river there had been nearly 2,000,000 cubic yards of accretion. That statement, standing by itself, was of no value without cross-sections to show where the accretion occurred, and from the survey of 1880, and as might be expected, it was manifest that all that accretion had taken place on the high banks, outside the line of the training-walls, and not in the channel itself, where there would be a considerable amount of erosion and deepening. In the next five sections it was admitted that there was erosion and deepening, and the result was that for $8\frac{3}{4}$ miles below the training-walls the channel had been deepened and improved. That he thought would tend to show that however desirable it might be to carry the training-walls, as shown on the plans, down into the estuary and into deep water for the purpose of preserving the ports of Havre and Honfleur, for the mere purpose of improving the channel into deep water, it was not necessary to carry training-walls the whole way, but that the channel would be deepened the whole way if the training-walls were taken some few miles farther only than they existed at present. It was also stated at the end of this Appendix: "Accordingly the volume of accretion, between 1875 and 1880, amounts to over 40 million cubic yards." He had considered the figures given in the Appendix in every possible way, but was unable to arrive at the 40,000,000 in any shape; he would therefore ask the Author carefully to consider the quantities quoted from Mr. Germain's report, and to give some explanation as to how that quantity had been arrived at. The Author also quoted from the same source: "Taking the divisions separately, instead of their sum, it appears from the above Table that accretion occurred in the wide division next the training-walls" (he had already pointed out that although there was accretion there, in all probability it was not in the channel itself); "then erosion in the next three divisions, followed by accretion in all the other divisions except the last." Whereas in the Table it appeared that there was erosion in the next five divisions, and then accretion in all the other divisions. He would ask the Author to reconsider this statement.

Mr. Shoolbred. Mr. J. N. SHOOLBRED said he would make a few remarks on the part of the Paper relating to the estuary and to the tidal portion of the river; not merely because he had had a long acquaintance with the Seine, but because circumstances had enabled him to obtain information, which he thought would be interesting to the members. Eight or nine years ago, as the Secretary of a tidal committee of the British Association for the Advancement of Science,

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charged with obtaining certain tidal observations in the English Channel, he was, by the permission of the French Minister of Public Works, placed in communication for that purpose with a number of Government engineers round the coast, at Havre, and several other places; and since that time he had been in receipt of certain official information, some of which, he thought, would be useful in connection with the Paper. From the Paper under discussion, a good idea could be formed of the history of the construction of the so-called walls, but which were mere mounds of rubble-stone. With regard, however, to the magnitude of the operations and changes which had been, and were being wrought by nature, principally through the disturbing influence of those training-walls, he did not consider that the information given in the Paper was sufficient to enable a due appreciation to be formed, on several points, as to the magnitude of the resultant effects. Mr. Shoolbred.

The longitudinal section (Plate 4, Fig. 2) of the part of the river from La Mailleraye to the sea, was in his opinion liable to convey an impression of a most marked improvement having taken place, not merely in the deepening of the bed, but also in the general condition of the river since 1824. That longitudinal section should, he considered, have been extended up to Rouen, near to the head of the tidal establishment of the river, as was shown in the one submitted by himself (Plate 5, Fig. 13). It would there be seen, that in the upper part there had been no similar artificial deepening of the river-bed. In fact, the excrescence in the river-bed, in 1824 and until 1853, between La Mailleraye and the sea, must have had the effect of a huge dam in penning in the water between La Mailleraye and Rouen. The effect upon the tidal-action, consequent upon the removal of this dam, was detailed, in part in the Paper. But to understand the changes more fully, there should be shown upon the longitudinal section of the river a comparison of the high-water and the low-water levels respectively, before as well as after the lowering of the river-bed (Plate 5, Fig. 13). It would be there seen, amongst other matters, that the ebbing tide, from the upper part of the river, had before the dredging been practically dammed back until reaching Tancarville, almost at the Estuary itself; into which it then poured down a steep incline, and consequently with very considerable scouring-power. Now, however, since the lowering had been effected, the discharge of the tide took place over a much gentler and more lengthy incline, and practically with the same head from La Mailleraye, instead of from Tancarville. The distance in the latter case to Berville

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Mr. Shoolbred. being but 7 miles, as against 28 miles in the former case. No accurate estimate of the state of the river could be formed from the longitudinal-section alone, without taking into consideration the cross-section thereof at various points.

A series of comparative cross-sections of the river, extending from La Roque to La Mailleraye, were taken by the engineers of the French Government in 1850, and again in 1867; before the commencement, and after the completion, of the river-walls. A certain number of these, taken from the more important ones, were shown as sections D, E, F, G, and H, and were shown in Plate 5, Figs. 8, 9, 10, 11 and 12. There were also shown some other sections A, B, C, Figs. 5, 6 and 7, extending at intervals down the estuary from the termination of the river-walls to the neighbourhood of Havre. The periods of comparison for these estuary sections were 1863, shortly before the completion of the river-walls, and 1880, the last complete survey of the Estuary. He himself had compiled these estuary cross-sections from official documents of the French Government. An examination of all these cross-sections, taken in conjunction with the longitudinal-section, showed that, owing to the enormous accretions between La Roque and La Mailleraye, amounting to over 20,000 acres, the amount of tidal-water now discharged from the river on the ebb-tide was about one-third less than before the works were commenced. While, again taking into account the diminished and at the same lengthened incline down which this reduced amount was discharged, it might be roughly estimated that the scouring-power of the tidal-discharge at Berville now was only about one-sixth of what it had been previous to 1850. With so considerable a diminution in the scouring-power at the upper end of the bay, it could readily be understood how those enormous accumulations, covering at least 35,000 acres, had been caused in the Outer Estuary. It was, as even Mr. Leader Williams had pointed out, only after the completion of the river-walls, that the improvement in the seaward portion of the Estuary had ceased; as evidenced by the relative position of the contour-lines, of the deep-water soundings, visible on the charts between 1834 and 1875. From the latter date to the present time the position of these contour-lines had steadily, and at times rapidly, receded seawards. Between 1875 and 1880, the 10-metre contour-line (below the "Zero des Cartes Marines") receded seawards $2\frac{3}{4}$ miles; the 5-metre contour $1\frac{3}{4}$ mile, and the 3-metre contour $1\frac{2}{3}$ mile.

The Author had shown in Appendix II, on the authority of Mr. Lavoinne and of Mr. Vauthier, that the low-water area of

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the Estuary had been decreasing steadily since 1875; by which date the diminished scouring-power of the river began to be felt in the Estuary. But the most serious of the various results was that now going on in the immediate vicinity of the port of Havre, between the Amfard Bank and the entrance to the port, and westward of the Hoc Point. The accumulations in that region were, during the period 1875–80, at the annual rate of 6,500,000 cubic yards, while between 1880 and 1883, that rate had increased to 9,000,000 cubic yards per annum. Thus during the decade 1875 to 1885 at least 60,000,000 cubic yards had been deposited almost at the very entrance to the port of Havre; and at a rate of deposit which threatened to close entirely the entrance to that important and flourishing port. This was the price which France was now paying for the improvement in the navigation of the Seine up to Rouen and for making that city into the fifth port of that country. In doing this, France was sacrificing Havre, its second port; and one which was of far more importance to that country, and to Northern Europe than Rouen was, or ever could hope to be. Yet, with these facts before them, some of the speakers, Mr. Leader Williams among the number, would fain inquire, Where was the damage to the Estuary of the Seine, arising from the works undertaken for the improvement of the river up to Rouen? Mr. Shoolbred.

It seemed to be admitted on all sides, in France as well as in this country, that the river-walls on the Seine must be extended seawards to the mouth of the Estuary. Several proposals had been made from time to time, but that suggestion which appeared to find most favour in France was the one of the late Mr. Lavoinne, who occupied at the time of his death, and had done so for some years previously, the post of Chief Engineer in charge of the Seine between Rouen and the sea. He was well fitted, therefore, from his position, from his personal qualifications, and from his intimate knowledge of the subject, to form a practical scheme for the extension of the walls. The Author had put forth in the Paper a proposal of his own. The course of his northern wall there indicated might be said to be identical with that of Mr. Lavoinne —indeed the course taken of late years by the sea-face of the accumulations in the Estuary, as well as the set of the flow and of the ebb of the tidal-current of River Seine, almost indicated the convex form and also the direction of this northern wall. But it was in the seaward extremity of the extension of the southern wall that the Author differed from Mr. Lavoinne. Westward of Honfleur, the former followed the low-water line, which was

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Mr. Shoolbred. pretty permanent, round the coast to Trouville. While Mr. Lavoinne's scheme extended from Honfleur westward into the bay, taking advantage of a ridge of rocks, up to a well-known shoal called the Ratier. His object being to form a breakwater for the interception of the tide-borne detritus, coming from the coast of Calvados and the south-west, and thus to prevent an accumulation of deposit in his newly-regulated entrance-channel to the Seine. The absence of any such precaution in the Author's plan was a serious objection to it; indeed there seemed nothing in his scheme to prevent the continuance of the deposits, as at present, on the southern face of the proposed northern wall. The depositing ground, or catchment-basin, clearly intended by Mr. Lavoinne, was the space between Villerville and the Ratier shoal. It was also very doubtful whether a deposit there formed would ever, as the Author feared, jeopardize the approach to Trouville, since the tidal-current from the south-west would, when the bank of accumulations extended sufficiently seaward, probably sweep on from Trouville and past the seaward end of the Ratier shoal without leaving any deposit. He strongly approved of this part of Mr. Lavoinne's proposal, in contradistinction to that of the Author.

Inferences had been attempted to be drawn from the above-described operations as to the effect of training-walls upon the Upper Estuary of the Mersey, and the want of similarity of the latter river with the tidal-portion of the River Seine. In such a comparison, however, the present condition of the Mersey, untampered with in the Upper Estuary, should be compared with that of the Seine in 1834 and up to 1850, before the construction of the river-walls (Plate 5, Fig. 1); not with its present mutilated condition, as the comparison was usually made. The conformation of the Seine, in its reaches immediately above the Estuary, would be found, at the earlier date, to present much closer points of analogy with the Upper Estuary of the Mersey than the advocates of training-walls in the Mersey would possibly like to admit.

In his opinion the results arising from the construction of the river-walls on the Seine (results most unfortunate, on the whole, and increasingly so each year) were of a most instructive character to engineers. They pointed out most unmistakably, how ill-judged it was, in tidal matters, to disturb violently the balance of forces which nature had established; and also, how difficult it was—nay, almost impossible, as evidenced by the later efforts of the French engineers—to re-establish that balance, once it was destroyed.

One word on the subject of movable weirs. It was noteworthy,

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in actual practice, to observe the clumsy and tedious operation of the needle-weirs, on the Seine. Take the one at Martot for example. Contrast it with the tilting-weirs of Mr. F. Wiswall on the Irwell, near Manchester, and on the Upper Mersey, and which the Author had briefly referred to. In the former case, each needle was separately removed by hand, over the stream itself, along a foot-bridge, which remained in floods as a permanent obstruction; while in the latter, the tilting-gates were all controlled and worked from the banks. Furthermore, in case of floods, the gates, if not already opened, were automatically tilted over by the increased weight of water, leaving an unimpeded water-way across the entire width of the river, save for the slight obstruction of the cast-iron supporting-frames of the gates. The saving of time in opening or shutting the respective weirs must be immensely in favour of the tilting-weir. Mr. Shoolbred.

Sir James N. Douglass said the Paper was especially valuable as recording another instance of the efficiency of training-walls. Sir James Douglass had had nineteen years' intimate knowledge of the Seine Estuary, and he entirely agreed with the Author in the opinion that the extension of the training-walls was necessary, if a deep-water channel from the sea was to be maintained. The proposals for the improvement of the entrance were very conflicting. It was first demanded that deep water should be preserved at Havre, and next that deep water should be preserved at Honfleur; and at the same time it was expected that no accretion should occur at Trouville. That, however, was nearly impossible. All would agree with Mr. Leader Williams that it was a difficult matter to compare any two rivers, and he believed that the question had hitherto been discussed without really considering the main point, namely the connection of the Seine with the sea. Fig. 3 represented the Seine in its relation to the English Channel, from which it would be seen that the direction of the discharge of the Seine was exceedingly unfortunate. It discharged in a direction nearly parallel to the coast; it had in advance and opposed to it the land trending out to the northward, and terminating in Cape Barfleur, which necessarily caused a shallowing in the elongated bay into which the Estuary opened, because it interfered with the tidal-current and prevented the scour that would otherwise occur. Again, the Seine delivered in the face of the travel of the eroded material of the coast to the west of it. It was well known that a very considerable quantity of material was delivered on the southern area of the estuary, as shown on the present charts. In an old chart, made from surveys taken in Sir James Douglass.

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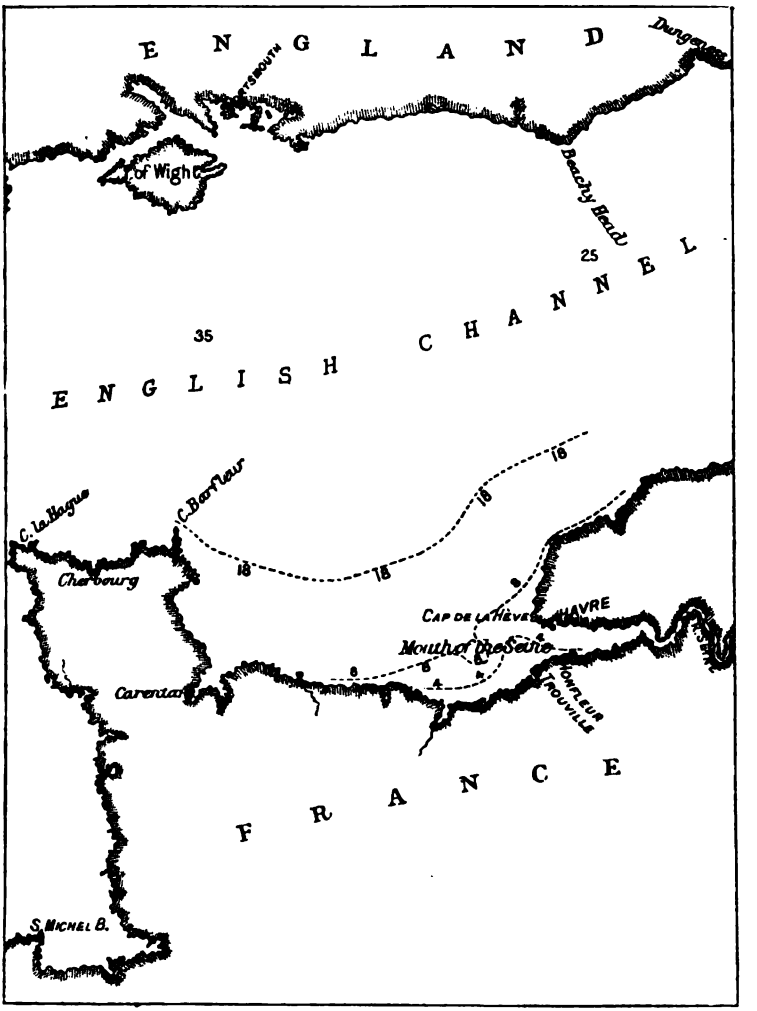
Sir James Douglass.

1776, it was shown that the deep water at that time was in front of Honfleur. Where it was then 9 fathoms it was at the present time only 2½ fathoms. Since that time the deep water had been

FIG. 3.



trending to the northward, and now it would be found by recent charts that the deep water was in the centre of the estuary, or nearly midway between Havre and Honfleur. Hence it might be concluded that the training-walls should be carried on nearly in

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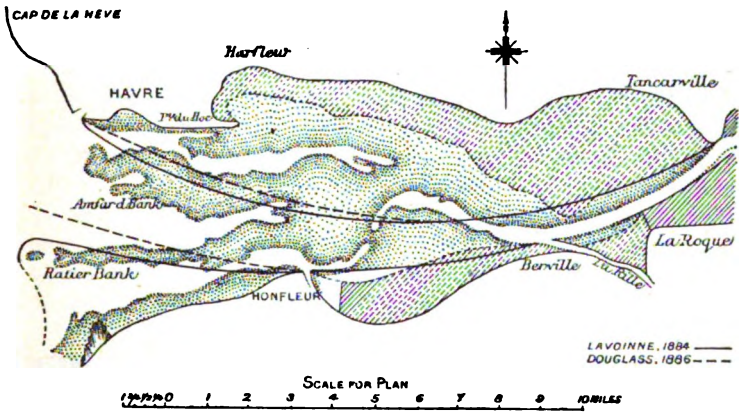
the direction indicated by Mr. Lavoinne, as shown in Fig. 4, the modifications he would suggest were shown in dotted lines. Evidence had been given that a very large amount of material had been brought down and deposited. Where was it to go? There was ample room for it in the Channel if it could only be carried there by narrowing the estuary. In doing so no doubt some accre- Sir James Douglass.

FIG. 4.



tion would follow at Trouville; but surely that was a secondary question as compared with the maintenance of deep water up to Havre, Honfleur and Rouen. He contemplated no difficulty in the problem, nor had he any doubt as to ultimate success. Not only would deep water be maintained, but nothing more would be heard of bores in the Seine.

Mr. E. H. PEMBER, Q.C., would not venture to give any opinion of his own upon the scientific questions involved in the discussion, but perhaps an unscientific by-stander might sometimes catch a point from hearing what was said on both sides. The remark of a previous speaker, who stated that he had had many years' knowledge of the Seine, had suggested to him what indeed had occurred to him more than once during the discussion on the Manchester Ship-Canal scheme, that the analogy between the Seine and the Mersey, when applied to the case of Liverpool, failed in one important point. Havre was not in a position on the Seine in any degree analogous to the position of Liverpool upon the Mersey. If there was one place on the Estuary of the Mersey which was analogous to the position of Havre on the Seine it was Formby, which was a very long way Mr. Pember.

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Mr. Pember. beyond Liverpool. Liverpool was placed upon the Mersey at a point which would be represented by some position to the north of Berville, on one of the lines representing training-walls or their prolongation. That appeared to afford food for considerable reflection in dealing with the question as a practical one. If the training-walls, whether prolonged to the point suggested by the Author, or stopping where they now did on the Seine, reduced the tidal-capacity to an enormous extent without interfering with the main sea-passage up into the river, or with the inland navigation, it appeared to him that, unless there happened to be a port on the Mersey in a bight, as Havre was on the Seine, it might be that no harm would be done by similar works on the Mersey. There might be any amount of accretion upon the Mersey, for instance, in the direction of Formby, but if it did not interfere with the sea-approach up in the direction of Manchester and Liverpool no harm would be done by it. If a lesson were to be learned from the Seine, and applied to the problem of the Mersey, the question would have to be answered, either in the affirmative or in the negative—had the works carried on upon the Seine injured the main sea-approaches into the Estuary? He had never heard that they had injured them. He had often consulted French pamphlets on the subject published by authority; and although a great deal had been said about the accretion in the neighbourhood of Havre, and also, he believed, in the neighbourhood of Honfleur, nothing had been said about the approaches to the river itself having been injured in any way. Mr. Shoolbred had stated that the price which France was paying for the improvement of her fifth port, Rouen, was the practical extinction of her second port, Havre. It might be true that Havre was damaged by the works carried out for the benefit of Rouen, but there was nothing in that analogous to damaging Liverpool by carrying out the proposed works on the Mersey. After all the question came to this, had anything occurred on the Seine, in consequence of the training-walls, analogous either to an injury to the Mersey Bar or to the approaches to Liverpool? The answer was, so far as he recollected the documents, that nothing had been done which was analogous to either of those things on the Seine. It had been proved to demonstration that the tidal-capacity of the Mersey had varied enormously within the last two decades. It had fallen since 1861 from upwards of 970,000,000 cubic yards to about 950,000,000 cubic yards, and it had risen to 960,000,000 cubic yards in 1881. That showed a decrease of between 20,000,000 and 30,000,000 cubic yards in the first decade, and a rise of

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8,000,000 or 10,000,000 cubic yards at the end of the second decade. During that period of twenty years it was proved to demonstration that the height of the bar had varied, and by no means in proportion to the variation in the cubical capacity of the river; in other words, that the cubical capacity of the river by natural causes had gone down very much, while the bar had not increased, but had perhaps diminished; and, on the other hand, the cubical capacity had gone up again, whereas the bar had not varied in the direction of improvement, or at all events in the same proportion. He ventured, therefore, with the greatest deference, to suggest that up to the present time two things seemed to be in great uncertainty: First, whether, after all, the cubical capacity of an estuary had really that direct effect upon a bar at its mouth which some engineers thought it had; and secondly, whether any effect had been caused by the works upon the Seine analogous to the damage of the bar on the Mersey, or to any port in the position of Liverpool. Mr. Pembor.

Mr. J. H. TAUNTON observed that as reference had been made by the Author to the Thames, the Metropolitan river, and as some slight comparison had been instituted between that river and the corresponding river of France, he wished to make a few remarks on the subject of inland navigation upon them as treated in the two countries. Before doing so he would consider how far the two rivers assimilated, and how far they differed. Undoubtedly the French river was much the larger of the two, its drainage-area being 30,370 square miles, and that of the Thames 5,172 square miles. The daily summer flow at Paris was 3,333,000,000 gallons as against 400,000,000 or 500,000,000 gallons in the case of the Thames at Teddington. The length from Montereau to St. Aubin was 197 miles, with a fall of 150 feet, and twenty-one or twenty-two locks. From Lechlade, to which point the Thames had been navigable from time immemorial (but where it had been rendered unnavigable by the Board of Conservancy, whose duty it was to take care of the navigation), to Teddington the distance was 126 miles, the fall being 220 feet, and the number of locks thirty-nine. The geological conditions of the two rivers were remarkably similar; they both passed largely through the upper beds of the Secondary strata and through the Tertiary strata, in consequence of which there was a great deal of permeable ground, as had been shown by the Author, and there was a still larger proportionate quantity of permeable ground in the basin of the Thames, which rose mainly in the Cotswold district amongst the loose rocks of the Oolites, the waters being maintained by the Lias Mr. Taunton.

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Mr. Taunton. and partly by the Fuller's Earth, and passing through highly permeable strata; hence there was not in the Thames the same torrential character as in the case of the Seine due to the Marne and the Yonne. The affluents of the Seine rose from a source, 2,960 feet above the sea, while in the Thames nothing came from above 700 feet. The Author had stated what had been done for the navigation in France, and it would be well to consider what had been done for the navigation in England. In 1866, in consequence of the bad condition of the Thames, an Act of Parliament was passed creating a new Board of Conservancy, and giving fresh powers for the maintenance of the navigation. Funds were also created, which were now somewhat large. The Board at present received upwards of £12,000 a year from the Water Companies of London alone, in addition to the tolls, and the total annual amount received for the maintenance of the upper navigation was above £17,000; but what had the Board done? In the first place, between Lechlade and Oxford in 1866 there was something of a navigation; there were some old movable weirs of a very primitive construction, but serving the purpose required, enabling boats to travel down the river from the canals joining the Upper Thames, and to reach London. Those boats brought, amongst other things, a great quantity of stone used for the Houses of Parliament. The weirs, which it was true were of a prehistoric nature, but were still of service, had all been removed by the Conservators, who took credit to themselves for having done so, and at the present time that part of the river was not navigable at all. He had himself a steam-launch for many years which he had run upon the river between London and Gloucestershire, and he had always found the greatest difficulty and some risk in passing as far as Oxford, in that it took him as long to get to Oxford as it did to get from Oxford to London. That state of things certainly was not satisfactory, and to add to it a new government had been instituted in connection with the Thames—that of the Thames Drainage Commissioners, who had powers over the river extending to somewhere near Clifton Hampden. Probably they were doing a useful work in supplying funds to the Thames Conservators, but in regard to the interests of the navigation the public would feel much happier if the arrangement led to some result, which up to the present time it had not done. There was a very loud outcry that a navigation which had existed for so many years—from the time of the Conquest—and in respect of which legislation had taken place in the time of King John, should have fallen into desuetude. It was not for want of funds, because funds existed, and erec-

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tions of a sufficiently serviceable character might easily be made so as to keep up some continuity in the Thames navigation. It was not for want of water. At Teddington, where there was a discharge of 450,000,000 gallons a day, one of the large locks described by the Author (197 feet long by 27 feet by 9½ feet, giving a capacity of 50,530 cubic feet), would be filled in a minute; at Oxford it would be filled in four and a half minutes, and even at Lechlade, where there was a discharge of about 20,000,000 gallons a day, it could be filled in twenty-two minutes, so that three locks might be filled in an hour. Of course the Thames locks would not be of that magnitude, but if they were of a sufficient magnitude to float barges carrying 100 tons, that would amply fulfil the purpose required. Many efforts had been made to re-open the navigation, to induce the Thames Conservators to do what it was their manifest and appointed duty to do, and as the Institution of Civil Engineers was to some extent a representative body, he hoped the discussion of the subject by its members would lead to something being done for the resurrection of the old Thames navigation. As to the floods on the Seine, between 1872 and 1884, occurring during the cold season, although the rainfall was then less than in the summer months, he thought that in those results would be found a confirmation of the views of the late Mr. Charles Greaves, M. Inst. C.E., who in his Paper "On evaporation and on percolation" said: "The yield of springs, or the abundance of water in a river, where the river is fed from springs, will be found, from a comparison of the tables .... to be dependent more closely on percolation through the soil than on the mere rainfall; indeed it will be found to correspond with percolation. For, whereas the variations in annual rainfall are as 2·33 to 1 ..... the annual percolation through ground varies as 3·5 to 1."[1] He thought that was an experience to be met with not only in the Seine but in all rivers in similar latitudes. Mr. Taunton.

Mr. A. GILES, M.P., thought that the chief interest of the Paper to marine engineers would be found in its reference to the entrance to the Seine at Havre. As to training-walls, it was only to be expected that the erection of such walls as those described would have the effect of scouring out the channel and vastly improving the navigation in the upper reaches of the river. That was pretty well shown by the section in Plate 4, Fig. 2, where the upper line indicated the old bed of the river before the training-walls were put in, and the lower dark line showed the improvement in the Mr. Giles.

[1] Minutes of Proceedings Inst. C.E. vol. xlv. p. 24.

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Mr. Giles. river-bed since the erection of the training-walls. The Author had stated that there were some very ingenious weirs on the upper portion of the river, and that floods could be predicted, so that the attendants at the weirs could be always ready to provide for them. He could only say that although it might be very desirable to predict a flood (which it appeared was four days in coming down from the upper to the lower reaches), he thought that with all the engineering science and invention of the present day there ought to be no attention necessary to alter a weir, his belief being that weirs might and would be made self-acting. A great deal had been done, as far as he could judge, by putting parallel training-walls along the Seine; but in looking at the plan from Havre to La Mailleraye, he thought that if the French engineers had devoted some of their surplus funds to cutting off the bends and making the channel more direct, it would have helped the navigation. It was evident that the training-walls, when raised to a height shown on the sections, must have the effect of reclaiming the land behind them, and that was indicated by the dotted lines. The Author had stated (p. 252) that: "The modifications needed in the width between the training-walls of the Seine will serve as a useful warning to prevent the tidal-flow of the Ribble from being similarly curtailed." The Author had not stated what those modifications were, but the inference appeared to be that the training-walls were perhaps too close together. He had also stated that the training-walls, carried out as they were in the river opposite Lytham, would only cause a sand-bank to accumulate at the foot of them, and that they must be carried farther. Mr. Giles agreed that they would have to be carried farther; but it was well known that when a Bill was being got through Parliament, it was not always desirable to indicate the work to the fullest extent. The Midland Railway was a case in point. It was not completed as a whole; it began in the Midlands, first getting its head and then its tail, and now it was one of the most magnificent systems of railways in the kingdom. So, in the case of the Ribble, when the works carried out were found to be successful, there would be no difficulty in carrying them farther. With reference to the mouth of the Seine, he thought that the French engineers were aiming at too much. He did not think it possible, however the training-walls were built, with the small volume of water coming down the Seine, to serve the ports of Havre and Honfleur with the same walls. It could only be done by bringing a curved channel, the hollow part of which should touch Honfleur, and then carrying the same curved channel almost opposite Havre. In that

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way there would be deep water opposite Honfleur, and also at Havre; but then Trouville would be sacrificed, because there could be no doubt that if training-walls were put from Honfleur in the direction of Havre, all the estuary to the west of Honfleur must gradually silt up. He was sorry to find that the Mersey had been dragged into the discussion, but he thought that those who represented the interests of the Manchester Ship-Canal should be able to defend what they had defended for two years in Parliament. He would assume, for the sake of illustration, that the bed of the great Estuary of the Mersey, called the Bottle above the neck opposite Liverpool, should be of solid rock, in which case he maintained that if the channel, as designed for the Manchester Ship-Canal, had been cut through the middle of the rock, without any training-walls whatever, that estuary would no more have silted up after the cut than it did before. He could understand the fear that if training-walls were put above the level of the sands at the sides of the cut, there would inevitably be reclamation and accretion. He never went farther than this, that if a cut were made, it should be protected in that soft material by walls, but those walls should not go above the level of the sand adjoining. He might cite Southport as an instance in point. It would be seen in the map that opposite Southport there was a spot called the Bog Hole. Formerly a channel of the River Ribble went through the Bog Hole, and when this was open, at low-water, the channel had a depth of 3 or 4 fathoms. The channel had been closed for some years with sand; yet the hole was now 6 or 7 fathoms deep. He did not think it could be argued from that circumstance that the channel was necessary to keep the hole open, since it was deeper without the channel than before. Mr. Giles.

Mr. J. B. Redman had intended to institute a comparison between the two national rivers of France and England, but he had been anticipated by Mr. Taunton with regard to the upper portions of the rivers. Although the rivers had many features in common, they had also some features that were very widely different. The main channel of the Seine, exclusive of its tributaries, was about double the length of the Thames, 400 miles as compared with 200; the area of the catchment-basin of the Seine was six times that of the Thames, 30,000 square miles, as compared with 5,000, and, excluding the permeable strata, it was about quadruple, 22,000 square miles. The volume of land-water passing through Paris, as compared with that passing through London, was proportionate; but then it should be remembered that London was situated upon a big tideway, while the tide fell short of reaching Mr. Redman.

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Mr. Redman. Paris by several miles. But even in Paris the river, like the Thames in London, had broken through restraint, for quite recently, in endeavouring to obtain an easier bed for its channel, it had undermined some of the foundations of the historic bridge, the "Pont Neuf." The Author had glanced slightly at the necessity hereafter of increasing the Parisian channel by enlarging the bridges, and Mr. Redman thought that a further enlargement might yet be brought before the consideration of the French Government, not only an enlargement of the bridges, but an elongation. In order to do that it would be necessary to set back one of the embankments of the Seine through Paris, and every one acquainted with Paris would know that the breadth of the Seine quays was excessive as compared with the channel between them. A similar action had been displayed on the Thames only a few years ago, in undermining the foundations of the old bridges at Westminster and Blackfriars. He would next direct attention to the very diverse condition of the tidal portion of the Seine, as compared with that of the Thames. Not only was it diverse, but it was absolutely the reverse. The tidal length of the Seine, 90 miles, was one-seventh of the entire length of the river; the tidal length of the Thames, 66 miles, was one-third the length of the river. The tidal oscillation of the Thames in London was 25 per cent. greater than in the estuary, 20 feet as compared with 16 feet; the tidal oscillation at the outfall of the Seine was twice the tidal oscillation at Rouen, 20 feet as compared with 10. The surface of the water at low-water in London was considerably lower than it was in the Estuary with certain tides; the surface of the low-water in the Seine at Rouen was 10 feet higher. The surface of the water at high-water in Westminster was from 4 to 5 feet higher than at Sheerness; the surface of the water at Rouen at high-water, as compared with Havre, was identical according to the Author's diagrams, Plate 4, which he had no doubt were correct. The tidal oscillation of the Thames, as compared with the Seine, showed that the first was in a progressive condition, and that in the last it was in a stagnant condition, and there was no need to go far to seek the cause. In the approaches to the Thames, and in the lower reaches of the river, there was a minimum depth of 20 feet at low-water spring-tides over the shoals, but the Seine Estuary at low-water practically ran dry. Mr. Vauthier, the French engineer, in his report upon the Estuary of the Seine, and the port of Rouen, had quoted some figures from a Paper read by him in 1877 on "The River Thames"; giving the elevation at high-water in London as 4 feet above the same high-water in the Estuary, which Mr. Vauthier regarded as a

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very remarkable result, and no doubt it was so to an engineer specially acquainted with the Seine. Mr. Vauthier had described him in the report as the Engineer having charge of the River Thames; he had certainly never told him so, nor could such an inference be drawn from his Paper, but perhaps it was assumed from the fact that he had brought forward statistics on the subject. As everything in France was to a certain extent official, it was probably thought that he must have some official connection with the Thames. Comparing the two estuaries, the anomaly was that the offing depth in the sea was as great opposite to the Estuary of the Seine, as it was opposite the Estuary of the Thames; the tidal rise at Havre was considerably greater, 20 feet as compared with 16 feet at Sheerness. In a discussion on a Paper read a few years ago by the Author upon tidal harbours intersecting the continental seaboard of the English Channel, and North Sea, of France, Belgium, and the Flemish countries, Mr. Redman ventured an opinion that the outfalls of those tidal harbours were as much affected by the flood as by the ebb, and that accretion on either side of the harbours arose as much from the tidal currents as from the wind-waves. He was then met by the objection from Sir John Coode, that it could hardly be so because the force of the sea-wave was fifty times that of the tidal-current. But thirty years ago he had read a Paper on the South Coast of England, and ten years later another Paper on the East Coast, in both of which he had shown that the great motor in moving the shingle deposits that lined the coasts, was the wind-wave. All along the continental seaboard of the English Channel, and the North Sea, the littoral detritus was entirely different; it was a white flocculent sand easily acted upon by the tidal-currents. The tidal-currents were constant; the action of the wind-wave was intermittent, and doubtless the detritus was as much acted upon by the tidal-currents as by the wind-waves. Sir James Douglass had referred to the influence of the promontory of La Manche upon the Estuary of the Seine, and had described the manner in which that promontory arrested the detritus travelling to the westward, but he did not refer to the remarkable tidal results produced by that peninsula, with which, no doubt, he was thoroughly acquainted. La Manche, 50 miles west of the Estuary of the Seine, projected 25 miles almost at right-angles to the general trend of the coast eastward; it projected almost like a natural groyne or pier, and it had a similar effect upon the French coast, as the Isle of Wight had upon the English coast. After high-water, when the tide set to the westward, it was dammed back by La

Mr. Redman.

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Mr. Redman. Manche to Havre. Havre, like Southampton, enjoyed a period of from two to three hours of still-water, which was, of course, very advantageous for docking and undocking vessels[1]; but it was very promotive of the deposits of detritus in the Estuary of the Seine. The Estuary of the Thames on the contrary, projected in such a form that it received the tide from the North Sea, and from the English Channel; there was a period of suspense of only twenty minutes, or half-an-hour, after high-water; the ebb-tide then bifurcated northwards to the North Sea, and south-west to the English Channel, and there was not the same time for the promotion of deposit. He cordially endorsed the remark of Mr. Leader Williams that, apart from the mere question of engineering, the study of a river was most interesting and fascinating. Dr. Robinson in the article on rivers in his "Mechanical Philosophy" spoke of the manner in which the ancients showed their indebtedness to rivers by absolutely deifying them and giving to each a presiding divinity. And the action of rivers, the circulation of the waters, the evaporation from the ocean, the condensation of the clouds, the distillation of rain, and the return by river channels, had been alluded to in a few terse words by a great eastern poet nearly three thousand years ago; he referred to Ecclesiastes, ch. i., v. 7. "All the rivers run into the sea; yet the sea is not full; unto the place from whence the rivers come, thither they return again."

Mr. Fox. Mr. CHARLES DOUGLAS FOX said that reference having been made in the Paper to the River Ribble, in the Estuary of which stream Mr. Brunlees and he had for some years been together engaged upon the reclamation of land for a private owner, some facts relating thereto might be interesting. For most of the observations upon which these notes were based, the Engineers were indebted to Mr. Thomas Ogilvy, of Rufford. An intake of 700 acres of land had been made in 1862. Since the completion of this work, the accretion on that part of the foreshore had been much more rapid, owing probably to two causes. The sea having been excluded from the enclosed portion, the area of 2,000 acres, or thereabouts, between the embankment and the low-water channel, was relieved from the action of the receding tide, which

[1] "At Havre, on the French coast, the high-water remains stationary for one hour, with a rise and fall of 3 or 4 inches for another hour, and only rises and falls 13 inches for the space of three hours; this long period of nearly slack water is very valuable to the traffic of the port, and allows from fifteen to sixteen vessels to enter or leave the docks on the same tide."—"Tide Tables for the British and Irish Ports for the year 1886." Computed by Staff-Captain H. R. Harris, R.N., Hydrographic Department, Admiralty, page 111.

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previously flowed over the 700 acres, so that the solid matters carried up by the flood-tide, and deposited at high-water, were not to the same extent removed by the ebb. During the last thirty years, the volume of sewage brought down by the river from Preston, and other towns, had greatly increased. The sand borne in by the flood-tide, rushing up the low-water channel, and over the lower sands, became mixed with this sewage, and thus was rendered more cohesive and more difficult of removal by the ebb, the result being, that the sands were raised more quickly to the samphire and grass-bearing levels. This sewage also added very much to the fertility of the sands, and, when they reached the grass-bearing level, the grass grew more rapidly and in greater profusion, and so caught more of the *débris*, which covered it at every tide. The layers of vegetable matter to be decomposed were consequently thicker, raising the level of the foreshore, and increasing the richness of the deposit. It had been observed that, in the Ribble Estuary, samphire grew on sands which were beyond the reach of all tides of less than 14 feet 6 inches above Old Dock Sill at Liverpool, or about 10 feet 6 inches above Ordnance Datum, and that grass would grow when beyond the reach of all tides less than 15 feet above Old Dock Sill, or 11 feet above Ordnance Datum. High-water level at the point in question varied from 6 feet to 17 feet above Ordnance Datum. During the present year one hundred and ninety-seven tides would not rise to 10 feet 6 inches above Ordnance Datum, and two hundred and forty-two tides would not rise to 11 feet above that level. Thus samphire would grow where 27 per cent. of the tides of the year did not cover the sands, and grass would grow where 33 per cent. of the tides did not reach it. When once the grass had started, the rise in the level went on rapidly till it reached about 12 feet 6 inches above Ordnance Datum; after that progress was much slower, as so few tides exceeded that level. When the embankment of 1862–63, which reached from the river Douglas to the boundary between Hesketh and North Meols, was finished, there were about 150 acres of green marsh left outside of it, near the river Douglas. All the rest was bare sands. In 1880, when the new enclosure was commenced, more than 1,200 acres were covered with grass. The second enclosure had reclaimed 1,077 acres, the embankment being about 3¾ miles in length, having its top 24 feet 6 inches above Ordnance Datum, or about 12 feet above the level of the sand, and 7 feet 6 inches above the highest tide (Fig. 5). This embankment was formed of the sand, protected by sodding, the sods being cut from the marsh outside the bank, and had a width at the top of

Mr. Fox.

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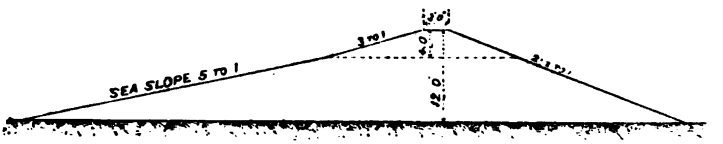
Mr. Fox. 3 feet, with slopes of 5 to 1 outside, except the top 4 feet which had a slope of 3 to 1, and 2½ to 1 inside. It had withstood with perfect safety the very high tides and severe storms to which it had been exposed. Mr. Fox would, however, prefer in future a width at the top of from 4 to 5 feet. The total cost of this reclamation had been about £15,000, or £14 per acre. About 800 acres had been already let at £3 10*s.* per acre, and the remaining 277 acres were being let at £3 per acre. The crops of oats during 1885, the first year of cultivation, had been abundant. A large tract of sands lying below this reclamation was now ripe

FIG. 5.



RECLAMATION EMBANKMENT, RIVER RIBBLE.

Scale 24 feet = 1 inch.

for enclosure, and the works would probably be commenced this year. The low-water channel of the Ribble had been much improved by the training-walls (rising a few feet above low-water) so far as they extended. Previous to their construction, and when they had been allowed to fall out of repair, the channels of the Ribble, and of its tributary the Douglas, were constantly shifting, as in the Dee and the Humber. The training-walls had, however, undoubtedly hastened the accretion on the foreshore, and the reduction of the tidal-area; and it was evident that if the channel was to be satisfactorily maintained, they must ultimately be extended seaward beyond the limits at present authorized.

Sir Robert Rawlinson. Sir ROBERT RAWLINSON, C.B., wished briefly to compliment the Author on the clear way in which he had brought the subject of the Paper before the Meeting. There was, however, one point connected with works of that description which ought never to be forgotten, namely, that French engineers were appointed by the Government, and that the works were executed by the Government out of Government funds. They were therefore not necessarily in their character commercial undertakings, there being very probably little consideration as to whether an adequate dividend would be returned for the money expended or not. In England the opposite method prevailed. Englishmen chose to

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Sir Robert Rawlinson.

do their works in their own way, Government doing very little for them' except sometimes making it both difficult and costly to get a Bill for any useful public work passed through the two Houses of Parliament. Then after this Bill had passed, the English engineer was left, with his Company, to his own devices. Works of the character described in the Paper had been carried out in England, not however upon such a scale as the improvements of the Seine and its tributaries, but exclusively for commercial purposes, and those purposes had been answered. The question for consideration in future, remembering that time was one of the great elements in modern civilization, would be, whether works like those completed on the Seine would be worth the cost involved in their execution and maintenance. He did not know any English work that could be compared with the works on the Seine, except, perhaps, the Caledonian Canal, which was a Government work; and although this fine work had been carried out under favourable circumstances, like a good deal of the French work it did not pay. The future of canals and of river-improvements would, in his opinion, be a very limited one, chiefly because the cost would be too great for the results proposed to be obtained. The information relating to the hydrology of the Seine was very interesting. A contrast had been drawn between the Seine and the Thames. Having been chairman of the Royal Commission for inquiring into the condition of the Thames, he necessarily knew something of the subject, as it had been his duty to examine the river minutely from its rise in springs to its estuary. It had been navigated rudely "time out of mind"; but the first Acts passed to bring it into a more modern form of navigation were in the reign of George II. The works under these Acts were carried out by Commissioners, whose powers passed into abeyance when the new Conservancy Board was appointed. This Conservancy Board had been blamed for letting the upper part of the Thames go to ruin, and for not having spent sufficient funds upon it. When, however, the care of the Thames passed into the hands of the present conservators, their income was very small; and their present income they owed in some measure to his exertions. When the Act was passed, he proposed in the Committee that the Water Companies should pay a rent for the water abstracted to maintain the Thames and to improve it, and, as it was the great source of water-supply to the Metropolis, to cleanse it; and the better to enforce his meaning, he also introduced the phrase to "scavenge" it, for at that time it was most disgracefully filthy, carcases of animals floating about on it, and weeds multiplying until they impeded

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Sir Robert Rawlinson.

the flow and corrupted the water. At that time the Water Companies were taking daily many millions of gallons of water from the Thames for commercial purposes, but paid little for it. One of them, he believed, paid £1,000 a year, and some did not pay anything. He proposed that each Company should pay at a rate of 6*d.* per million gallons of water taken by it; but certain Members of Parliament, with a view to popularity, opposed that mode of taxing the Companies. He, however, told the Companies then, as he should tell them again, that for their own security it was advisable to get statutory rights to take the water to pay for those rights, and then to scavenge and adopt and carry out works and means to protect the river from abuse and impurities. One or two Bills had since been passed to increase the contributions, and the Conservators now had, he had just learned, an income of £17,000 a year. The reason why the Thames decayed under the superseded Commissioners was that they got into debt. When they were put on one side by the Bill which arose out of the report of the Rivers Pollution Commissioners, the traffic did not pay, and the opening of railways rendered improvement for commercial purposes hopeless, and as they were £80,000 in debt, they did not see their way to safely increase that debt, and so their powers lapsed, and every farthing had been lost.

The statement as to rainfall was very interesting, and he wished to make a few remarks on that subject, and as to the volume of water flowing off the ground. The Author had stated that there was a greater rainfall in summer than in winter, and that a less volume of water flowed from the ground in summer. That was easily accounted for. In summer, though there was a larger volume of water, if the rain did not come down for several weeks continuously, although the season might be a wet one, very little water would be given to the springs and watercourses. In dry summer weather 2½ inches of rain might fall within seven days, and not a drop would show itself either in spring or stream. Unless rain fell continuously week after week in summer, very little water was added to the streams. As to permeability, if it was a question of excessive floods—those that did the greatest mischief—it mattered very little whether there was a large area which was permeable, or a large area which was impermeable, for the one under continuous heavy rain became exactly the same as the other, as when the permeable stratification had been filled, as it were, up to the brim, the water falling on it subsequently ran off as from a slated roof, and when there were long continuous rains month after month, the proportion flowing off the surface

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throughout the year might not be more than two-thirds of the rain which had fallen. The Author had stated that the average rainfall in the Seine basin was 27·24 inches, and he had given both the dry-weather and wet-weather flow. Sir Robert Rawlinson had been able to obtain a rough and ready mode of arriving at a minimum and maximum of a district, if he could only get a series of rain-gauge tables sufficiently long. Taking an average and dividing it by three, then if one-third was deducted, it would give the approximate dry-weather flow, and if one-third was added, it would give the approximate wet-weather flow; the ultimate result being that a dry season was to a wet season about as 1 to 2. Sir Robert Rawlinson.

In the English climate, the order of seasons was different from that popularly imagined; spring was the driest season, and April was the driest month; autumn was the wettest seasor, and October and November were the wettest months. A flood would discharge from a given surface five hundred times the minimum volume of water in a dry season, and in some excessive cases, a thousand times. Those were facts that all young engineers having to deal with water should thoroughly comprehend, as it was that excessive variation in the volume of water flowing down natural channels which made river-improvement so difficult and costly. The irregular contouring, due to the rolling, digging, and disturbing motion of a volume of water in flood coming down with considerable rapidity was evident. In a canal there were level reaches from lock to lock, and an even depth, and an even bottom could be maintained, only requiring occasional dredging to remove side-bank subsidence, uprising, and silting; but in a river like the Seine, the Thames, or any other where there were great floods, the bed was constantly exposed to an incessant disorganization, that was, the bed of the channel between lock and lock. Reference had been made to the possibility of taking sea-going ships to Paris. That dream would never be realized. The Seine was a much more important river than the Thames in regard to its hydrographical area, which was five times as great, and its length also was considerably greater; but the Thames was a hundred times more valuable for commerce. It was true that Paris stood upon the Seine as London stood upon the Thames; but from London Bridge to the sea there was a magnificent navigable channel up which the greatest ships that traversed the ocean could go, and there were docks unequalled in the world, except at Liverpool, to accommodate the shipping, so that for commercial purposes the Thames, in comparison with the Seine, had an enormous advantage; and if engineers did not learn something from these facts,

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Sir Robert Rawlinson.

they were wasting their time. He did not say that they might not learn much from reading the Paper and looking at the diagrams; but they would never learn all that was required unless they thought out for themselves the problems involved. When young engineers were called upon to construct works of that character, they should take fairly into account the fact that there were certain operations of nature which ought to be thoroughly mastered, otherwise the works would rapidly come to ruin. There were regions which Englishmen were colonizing, where the rivers rose 70 feet vertically in flood, and there were others where, for a series of years, the rivers were dry pools, and in floods rose 120 feet vertically. The improvement of navigation in such rivers would, he thought, be a very difficult problem. But there were other regions where there were great lakes that might be connected, great areas of country that might be opened out. There was, therefore, in the future plenty of work for ambitious young men. The Gota canal connected Göttenburg with Stockholm, between which places there were two magnificent lakes, the Wener and the Wettern, and useful as the canal had been, and now was, it had been found that in the present day, when time was money, it was necessary that it should be supplemented by a railway. He was not quite sure that some day it might not also be found necessary to supplement the Caledonian canal by a railway running parallel with it through the valley. As to the question of rainfall and evaporation, it was, he believed, Dalton who said that, taking the whole circumference and area of the globe, as far as it was known, evaporation might amount to 36 inches per annum. If that were so, precipitation must be an equal volume, the one regulating the other. The great source of evaporation was the salt ocean, and the great power causing evaporation was the sun. If the heat of the sun was regular year by year, which he doubted, there would be year by year the same amount of evaporation and the same volume of rainfall in precipitation. Evaporation and precipitation were unceasing over the entire area of the globe, from the tropics to the poles. Storms and rainfall were, however, unequal in time and partial in areas. Very little rain fell over flat deserts; excessive rain prevailed along the sides of mountain ranges. Seasons of extreme drought alternated over limited areas with seasons of excessive wet, with an unceasing evaporation. If, however, in the earth's revolution through cosmical space, vapours from the atmosphere were abstracted and lost in space, however minute the annual volume might be, yet the time would come when the ocean would be dried up, the atmosphere vanish, and the earth

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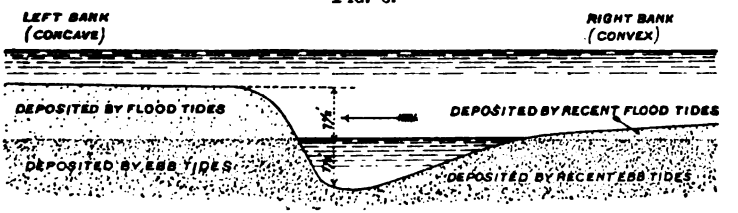
become rugged and crumpled-looking like the moon. Then would be the eve of the final catastrophe foretold by Hebrew prophets, and indicated by Shakespeare in the "Tempest," where Prospero explained to his astonished and wondering auditors the prophetic lines — Sir Robert Rawlinson.

> "The cloud-capp'd towers, the gorgeous palaces,
> The solemn temples, the great globe itself—
> Yea, all which it inherit—shall dissolve,
> And, like this insubstantial pageant, faded,
> Leave not a rack behind."

Mr. G. F. DEACON said the Author had stated that "the Seine works demonstrate that accretion is the result of training-walls, whether high or low, in a shifting sandy estuary." That passage had been the subject of adverse criticism, and as he felt satisfied of its truth he wished to consider it in the abstract, though for the sake of example, and to fix the ideas, it would be necessary to Mr. Deacon.

FIG. 6.



employ figures relating to a particular case. There was one law which he thought held good for all alluvial or diluvial plains, from the first flat-land traversed by the mountain stream, to the sheltered sands of some great estuary, namely, that the concave bank of the channel was always higher than the convex bank. If levels were taken along the two banks, that would be found to be absolutely correct, assuming always that the alluvium and the channel were left to themselves. The reason for this was obvious. As a specific example he had prepared a section, Fig. 6, representing the mean condition of the low-water channel in the 10 square miles of the Upper Estuary of the Mersey, included between a straight line drawn from Ellesmere Dock to Speke Hall, and a straight line drawn from Stanlaw Point to Dungeon Point. That section was typical of all rivers in alluvium, though in other cases the dimensions would not be the same. In the particular instance cited the circumstances were these:—There were, on the average, rather more than 4½ ozs. of silt per cubic yard of water brought from seaward by every tide. Supposing

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Mr. Deacon. the whole of that silt were deposited in the Inner Estuary, and not again removed, the rate of accretion at any point in the Inner Estuary would be about 1 foot per annum if the mean rise of the tide were 10 feet above the place of deposit, and much in the same proportion for other mean depths of water. On reference to the typical section, it would be seen that the left and steeper bank of the channel was 7½ feet higher than the right bank, which itself was 7½ feet higher than the bottom of the channel. The left bank was the concave bank; the right bank was the convex bank. By erosion of the left or concave bank, the channel travelled laterally towards the left at the mean rate of 450 yards per annum, and it passed over an area about equal to the area of the 10 square miles in question once in ten years. In moving laterally to the left, the water carried away the whole of the concave bank, 15 feet in height, the lower part of which was marked "deposited by ebb tides," and the upper part of which was marked "deposited by flood tides." Where did it take the material of that bank to? Manifestly it took just half of it away from that part of the river altogether and carried it towards the sea. The other half it deposited on the right bank, thus maintaining the width of the channel constant. The water in a river bend acquired, as had been well pointed out by Professor James Thomson, a helical motion, due to the centrifugal force being greater, owing to greater velocity, near the free surface than near the bottom of the stream. The resultant motion of the upper water was therefore down the channel and across to the concave bank, and that of the lower water down the channel and across to the convex bank. Mr. Deacon would add that by this motion the destination of the eroded material was determined, the finer particles being carried by the upper and middle water towards the sea, and the coarser particles being washed across by the lower water and deposited on the convex bank marked "deposited by recent ebb tides," to the height of the contained stream. Accordingly it was found, and this again was typical of all rivers in alluvium, that for the first 7½ feet above the bottom of the channel the material of the banks was coarser than in the upper 7½ feet on the higher side. As the low bank was thus built by the channel on the concave side, the tides overflowing that bank began to form upon it another bed marked "deposited by recent flood-tides," and this deposit being brought up chiefly as suspended matter from seaward, was of finer material than the channel-built bed below. On the concave side this tide-built bed had grown to a height of 7½ feet above the convex bank before the channel cut it down again. In

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the case of alluvium proper the corresponding effect was produced by land floods. He thought it must be obvious to everyone that the slower the lateral motion of the channel the higher would the flood-built bank become, and the higher would be the mean level of all the sands; and if the channel ceased to travel laterally, it was equally obvious that there was no limit except height of tide to height of accretion. The tide at present rose above the concave bank of that particular channel to a mean height of 13 feet, so that if all the silt were deposited there would be an accretion of 8·4 feet. There actually was an accretion of 7·5 feet, showing how large a proportion of tide-borne silt was deposited and only returned to the sea through the agency of the low-water channel. He had previously stated that if all the silt brought from seaward by the tides were deposited in the Inner Estuary and not removed, the rate of accretion would be 1 foot per annum for each average tidal-depth of 10 feet. Hence, having regard to the diminishing depth of water as the deposit increased, the corresponding depth of accretion in ten years would be 6·5 feet, and in the case in point, where the initial mean tidal-depth was 13 feet the accretion in ten years would be 8·4 feet. Mr. Deacon.

It had been stated by Mr. Leader Williams that "as there were sand-banks in the Mersey, particularly one at Eastham, through which the low-water channel had not gone for many years, and that bank had not increased above its ordinary level, it was evident that the natural powers there were sufficient to keep it down to that level." Mr. Deacon, however, could only find one bank of any importance, across which, during ten years, the channel had not travelled, and that was near the middle of the Estuary between Stanlaw Point and Oglet Point. In the decade from 1871 to 1881 a large portion of that bank was apparently not affected by the channel. The mean accretion during that period was 4½ feet. The mean rise of the tide above the height of the bank at the beginning of the period was 9 feet, so that, in this case, if all the tide-borne silt were deposited, there would be an accretion of 5·7 feet, while there actually was in that decade an accretion of 4·5 feet. It had been stated that because the Cheshire side of the Mersey was 5 per cent. longer than the Lancashire side, therefore the ebbing tide flowed down the Lancashire side; but the fact was that the ebbing tide did not do so. It took for its maximum velocity a path which was on an average 30 per cent. longer than the Lancashire side. The ebbing tide might have been expected to flow down the concave Cheshire side, towards which it was projected by the direction of

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Mr. Deacon. the Estuary immediately below Runcorn Gap. The reason why it failed to do so was because on that side the River Weaver and several smaller streams flowed into the Estuary, and they tended to form deltas and lateral banks, which, together with the momentum of the tributary waters, pushed the Mersey channel away; and thus although, *cæteris paribus*, its natural position was near the concave Cheshire shore, it was in fact ever changing, and on the average flowed nearer to the Lancashire than to the Cheshire shore. These conflicting influences were, he believed, the salvation of Liverpool; for if from any cause the Cheshire affluents ceased in this respect to perform their present functions, the low-water channel would undoubtedly tend towards the concave Cheshire shore, and would become less unstable, and nearly in proportion to its instability would be the resultant capacity of the tidal-basin, upon which the sectional-area of the bar-channel was dependent. He said sectional-area advisedly, in preference to depth, as much misapprehension had arisen from comparisons of depth alone. It was true, however, that the larger the channel, the deeper on the average it would generally be.

Mr. Barry. Mr. J. W. BARRY considered the Paper valuable in two ways, in one way as dealing specially with questions of great interest in connection with the description of the Seine, in regard to inland navigation, and in the other in considering certain general principles in the treatment of an estuary, and applying them to the special instance of the Seine. He would not refer to the details of the Upper Seine, but would allude briefly to those general principles which interested engineers in their application to an estuary. If the velocity of water was diminished, the matter in suspension, which corresponded to the velocity, would be deposited, and it did not much matter, when these principles were being considered, how the velocity was diminished. Again, if the velocity was increased in one part of the cross-section of a river, the velocity in the other parts of the cross-section would be diminished. Therefore, if training-walls were put in a river, whether parallel to the course of the river or at right-angles thereto, the purpose and certain result of which was increase of velocity locally, deposition might be expected. In the Mississippi, deposition had been intentionally encouraged by all kinds of ingenious devices by Mr. J. B. Eads, M. Inst. C.E., who had given instances in which the mere placing of nets or light hurdles in the river had checked the velocity of the stream, and caused considerable banks to be formed, which by narrowing the river had increased the navigable channel in depth, and tended to those great improve-

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ments with which his name was so honourably connected. If, on the other hand, a deep-water channel in the river was dredged, and the hydraulic mean depth thereby increased at one particular portion of the cross-section of the river, the velocity of the water would be increased in that portion, the velocity at other parts of the cross-section of the river would be diminished, and deposition would take place on each side of the channel. He therefore could not agree with the principle that had been laid down by some of the speakers, that unless training-walls were raised to the level of high-water or half-tide, deposition would not take place. He believed that training-walls, of whatever height they might be, tended to make a deep-water channel, and to increase the velocity at one portion of the cross-section of the river, and that they would infallibly tend to deposition at adjoining portions. Thus, it was not possible to lay down any general mode of treatment which was applicable to all rivers. Sometimes accretion was not of much consequence, and a deep channel was of primary importance. In these cases it would be possible boldly to adopt training-walls or jetties, or dredging, or any other mode of increasing the hydraulic mean depth of the channel. But in other instances accretion was of vital consequence; it might tend to the destruction of a port, which was dependent upon the flux and reflux of the tide to keep the bar open. Again, in some cases the increased quantity of tidal-water due to making or deepening a channel might, for scouring purposes, counterbalance the quantity of water excluded by deposition, while in other cases, and especially when a great part of the deepening took place below the level of low-water, the increased quantity of water in the channel would not be worth consideration. With regard to the Manchester Ship-Canal scheme, he thought the Author was perfectly justified in his statement, that if the principles he had laid down, and which he might observe were well known, had been sufficiently apprehended at the outset of the project, the two abortive attempts in Parliament to which he had referred would not have been made. He thought that every one had learned a great deal from those attempts, and whatever some engineers might think about it, Parliament, at any rate, was thoroughly convinced that the training-walls first proposed could not be put in the upper portion of the Mersey without the greatest risk of deposition, and the consequent exclusion of the tidal-water, which was admittedly necessary for the maintenance of the channel over the Liverpool bar. Engineers were able to say that training-walls would be very useful in de-

Mr. Barry.

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Mr. Barry. veloping a channel, but that a new channel could not be developed without decreasing the velocity in other portions of the cross-section of a river; and in recommending such works as training-walls, or cross-jetties, or any other mode of developing a channel, they should be prepared for deposition, which might or might not in each particular case be of consequence. The importance of deposition depended first of all upon the quantity of silt or sand held in suspension, and then upon the secondary results arising from deposition in the exclusion of tidal-water.

Mr. Vernon-Harcourt. Mr. VERNON-HARCOURT, in reply, said with reference to the additional information concerning the different classes of strata desired by Mr. Baldwin Latham, he believed this was somewhat indicated by comparing the two plans of the Seine Basin (Plate 1, Figs. 1 and 2), and noting, at the same time, the information given in the Table accompanying them, of the geological nature of the several districts. Further information, however, could be found in a coloured geological map of the basin, in Mr. Belgrand's book on the Seine,[1] which was also given in Mr. Préaudeau's manual;[2] but this map would have been too costly to add to the numerous illustrations accompanying the Paper. He had only referred to the new Seine weirs as possibly applicable to Indian rivers (p. 252), and not to the other works on the Seine, as General Rundall appeared to suppose. General Rundall had pointed out that the form of the basin might exercise an important influence on the floods of a river; and to this interesting consideration he would add, that the positions of the confluence of the tributaries, and the situation of the higher parts of a basin in relation to the sea and the prevailing winds, must also have a considerable effect on the floods of the main river. Mr. Taunton had described the defective condition of the Thames between Oxford and Lechlade, which he could fully corroborate, as he was intimately acquainted with that portion of the river, having taken numerous cross-sections along it, and traversed it several times for the Upper Thames Valley Drainage Commissioners. From information which he had received when consulted last autumn about the river near Oxford, he gathered that the Thames Conservators had been obliged to capitalize a large portion of their income in order to raise adequate funds for rebuilding numerous weirs, and also some locks, being aided in the works on the upper river by the Commissioners. Accordingly, the Conservators would in future

[1] La Seine, Études Hydrologiques, p. 27; in Atlas, pl. 1.
[2] Manuel Hydrologique du Bassin de la Seine, pl. 1.

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have no funds available for carrying out improvements beyond the works now in progress; and the Commissioners, who had certain powers of rating for drainage improvements, were not able to obtain sufficient funds to carry out all the works they deemed expedient. He feared, therefore, that the navigable condition of the Upper Thames could not be materially improved unless the Government took in hand the inland navigation of the country, a system adopted in France, India, the United States, and other countries. Sir R. Rawlinson's view that such works were generally unremunerative, might be true as regarded private enterprise; but it did not follow that the State would not gain by these undertakings. For the State derived an indirect benefit from the increased prosperity of the nation, and from improved facilities for trade, by the augmentation of taxable property; and this was considered in France, and probably in the United States also, to provide an ample return on the expenditure. Mr. Vernon-Harcourt.

He could not agree with Mr. Leader Williams in considering solid weirs the best for English rivers, though he thought that large weirs of the type of Poses and Port-Mort were too costly to be adopted in England. He was of opinion that improved forms of draw-door weirs, of which Mr. Stoney's design was an instance, Mr. Wiswall's tilting-weir, and the smaller and simpler forms of French movable weirs, might be adopted with great advantage on English rivers. An automatic system for weirs, which was advocated by Mr. Giles, had been tried at the earlier shutter-weirs on the Seine; but the working had proved uncertain; and, except for the butterfly-valves in the more recent shutters, the system had been relinquished in favour of a regulation of the flow directed by the weir-keeper, who could be warned, in his cottage, by an automatic bell, of a rise in the river necessitating his attendance. He was surprised that Mr. Shoolbred, who claimed to have some acquaintance with the Seine, should have fallen into the error of supposing that the footbridge of the needle-weirs was a permanent obstruction in the river; whereas he might have ascertained from the Paper on "Fixed and Movable Weirs," from Plate 3, Fig. 5 of the present Paper, and from the photographs exhibited of Port Villez weir, that the frames were lowered into a recess in the apron in flood time. It would have been fairer also to French engineers, in comparing their weirs with the tilting-weir, to have selected the shutter-weir, or the more recent forms of frame-weirs, as examples, rather than the needle-weir introduced half a century ago. The obstruction in the tilting-weir, when opened, (described as slight) constituted one-fifth of the

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Mr. Vernon-Harcourt.

whole waterway, for the frames and gates remained in mid-stream; but he considered this the only disadvantage in this form of weir, which in other respects was very satisfactory.

Several speakers had discussed at some length his allusion to the proposed training-works in the Mersey. A consideration of the results of the Seine training-works led him to express an opinion adverse to the improvement of the Mersey Estuary by training-walls, early in 1882, in his book on "Rivers and Canals," and in his Paper on "Harbours and Estuaries on Sandy Coasts," before the first Manchester Ship-Canal scheme had been brought forward; and he was confirmed in this opinion by an inspection of the Ribble Estuary for the Southport Corporation in 1883. A description of the objections to training-walls in the Mersey did not enter into the scope of a Paper on the Seine; and his views were well known, having been given in evidence before five Committees on the Manchester Ship-Canal bills. As, however, considerable prominence had been given to the question in the discussion, he had made a plan of the Mersey, showing the estuary-scheme for the canal of 1884. (Fig. 7, p. 309.) He had never suggested that there was any similarity between the Seine and the Mersey estuaries, as Mr. Shelford and Mr. Leader Williams appeared to imagine. A comparison of Plate 4, Fig. 1, and Fig. 7 sufficed to show how unlike they were in form. They resembled each other, however, in having large accumulations of sand, a considerable range of tide, and a shifting channel; and there was every reason for supposing that the results of training-walls would be similar in both estuaries. It had been suggested that the accretions in the estuary of the Seine were the result of high training-walls; but low training-walls were adopted below Tancarville and La Roque, and still accretions took place behind these low walls, and even extended 8½ miles beyond their extremities. Liverpool Bay was a natural receptacle for wave-driven sand; and the reclamations which had been accomplished by the training-banks in the estuary of the Dee to the south, and the accretion which had resulted from the low training-walls in the estuary of the Ribble to the north, showed how inevitably accretion must follow training works along that coast. Both Mr. Shelford and Mr. Leader Williams stated that the inner Mersey Estuary was a tidal reservoir, and they pointed out that the bar at the mouth of the Mersey rendered it different to the Seine. He fully endorsed these opinions, and considered therefore that training-works, which might be comparatively harmless to the outlet of the Seine, would be most injurious to the outlet of the Mersey.

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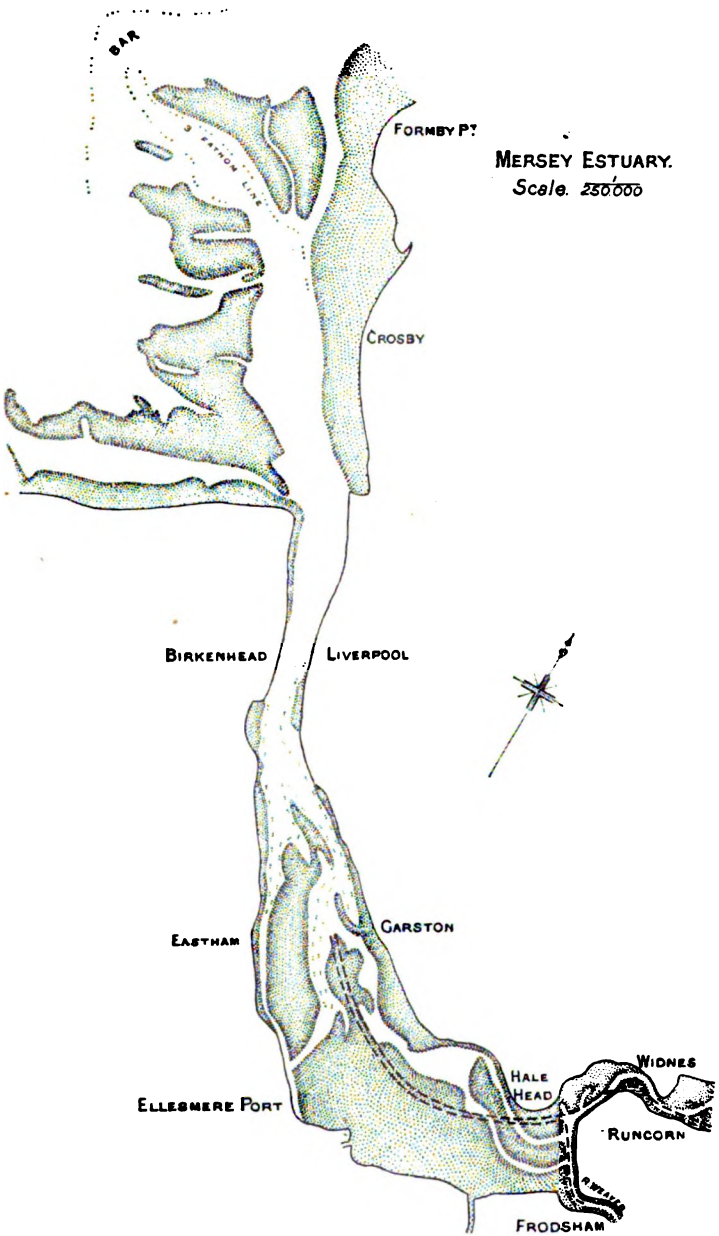
Mr. Vernon-Harcourt.

FIG. 7.



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Mr. Vernon-Harcourt.

It was precisely because he realized the important differences between the two estuaries, that he advocated the extension of the training-walls in the Seine, and opposed their introduction in the Mersey. Training-walls produced two effects, namely, the improvement of the channel between them by the concentration of the flow in one fixed course, and the accretion of land behind them owing to the diversion of the scouring influences from the rest of the estuary into the fixed channel; the first effect was a manifest gain, and was fully recognized by engineers; whilst the second might or might not be injurious, according to the form of the estuary and the position of ports or towns on the coast, and was liable to be overlooked. He was fully alive to the advantages of training-walls for affording access to inland ports, and had declined to oppose the extension of the training-walls on the Dee, as he believed that they might be prolonged in that estuary for the benefit of Connah's Quay and Chester, without injuring other places by the inevitable accompanying accretion. The maintenance, however, of the upper estuary of the Mersey, as a tidal reservoir, had been acknowledged by the engineers on both sides in the Manchester Ship-Canal inquiries, as essential to the preservation of the channel over the bar; so that the main controversy was limited to the question whether accretion would, or would not have resulted from the proposed low training-walls. Mr. Lavoinne, a zealous advocate of training-walls, had stated, in conversation with him in 1883, that he fully agreed with his opinion that accretion must follow from the proposed training-works in the Mersey. Mr. Thomas Stevenson, M. Inst. C.E., to whose opinions Mr. Williams had alluded, was reluctant at first to admit that training-walls, which he had employed with success in several estuaries, could have sometimes a deleterious effect; but after careful consideration, he fully acknowledged the correctness of Mr. Vernon-Harcourt's views about the Mersey, and gave his conclusions in his Presidential Address to the Royal Society of Edinburgh, on the 2nd of February, 1885,[1] in which he stated, speaking on the principal causes of silting in estuaries, that, if the canal estuary project had been authorized, it might "have ended in something very like a national calamity, namely, the silting up of the sea-approaches to Liverpool." Moreover, in his concluding remarks, speaking of the reduction of tidal capacity which would have been effected by the works, he said, "not one-twentieth, which Mr. Walker feared to authorize at the Tay—not

[1] Proceedings of the Royal Society of Edinburgh, vol. xiii. pp. 82, 91.

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one-tenth, as has actually taken place at the Lune—but one-half of the whole scouring-power would be excluded from the Estuary of the Mersey." Mr. Deacon and Mr. Barry had well described the natural causes which must produce accretion if a channel was fixed by training-walls in an estuary like the Mersey. Mr. Leader Williams and Mr. Giles adhered to their former opinion that low training-walls would only improve the channel between them without influencing the rest of the estuary, which he believed was contrary to the laws of nature, and was not in accordance with his experience of other estuaries. Mr. Shelford and Mr. Williams stated that the proposed Mersey training-walls reached deep water; but it was evident from Fig. 7 that this was not correct, for sandbanks extended in front of them, and the bar was beyond. Had the training-works been carried out, the accretion would have gradually crept down beyond them, closing up the channel to Garston Docks, and they would have needed extension to the narrows; the great reduction in tidal capacity would have shoaled the bar-channel, and works would have eventually been necessary in the outer estuary to regain the present depth. Accordingly, the authorized canal scheme, by not interfering with the estuary, was far better for Manchester, as it did not imperil the bar-channel; and, moreover, it did not endanger the approaches to Garston and Ellesmere Port. He was surprised that Mr. Pember considered the narrows the critical portion of the Mersey Estuary. He had never suggested that the narrows would be silted up by loss of tidal capacity above, but that the bar-channel would be affected, which was the vital point for Liverpool. Mr. Pember would see in Appendix III. that the main channel at the entrance to the Seine had decreased 23 feet in depth between 1875 and 1880. This reduction had indeed occurred in sufficiently deep water not to affect the access for vessels; but what would be the effect of any such diminution in depth in the bar-channel of the Mersey? Mr. Pember considered that it had been proved that the depth over the Mersey bar bore no relation to the capacity of the upper estuary. The influence, however, of the tidal capacity must be measured by the total sectional area of the outlet, and not by the maximum depth at any one point. No uncertainty, he believed, existed amongst engineers as to the effect of tidal scour on the Mersey bar-channel. The channel owed its existence to the tidal current, and would be obliterated if the tidal action ceased. The value of tidal scour in maintaining an entrance channel had been amply proved in his Paper on "Harbours and Estuaries on Sandy

Mr. Vernon-Harcourt.

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Mr. Vernon-Harcourt.

Coasts"[1]; and the deterioration of the outlet was shown to result invariably from a reduction of tidal capacity above. The only other influence at work in maintaining the outlet of a tidal river, against the heaping-up action of winds and waves along the coast, was the fresh-water discharge. The average fresh-water discharge of the Seine was about twenty-eight times that of the Mersey, whereas its tidal capacity was less, which showed how far more the maintenance of the Mersey outlet was dependent upon tidal scour than the Seine. Mr. Giles and Mr. Fox agreed with him as to the necessity of a future extension of the Ribble training-walls, beyond their present authorized limits, to maintain a navigable channel. Mr. Fox had also confirmed his statement that accretion and reclamation had resulted from the existing low training-walls. The prospect these conclusions held out might be satisfactory to the riparian landowners, who would be able to make further reclamations; it would hardly be equally so to the Preston Corporation, who, whilst greatly reducing the tidal capacity of the Ribble Estuary, would be obliged to undertake considerable extensions of the training-walls; and it would be very serious for Southport, a favourite seaside resort, which would be gradually deserted by the receding sea, and thus lose the very object of its existence. Mr. Giles had stated that the Bog Hole, at the head of Southport pier (Fig. 1), had increased of late years in depth. He thought that this local action was due to the extension of the pier-head. The channel, however, had decreased in length, and the hollows to the north of it were shoaling up. At present the Bog Hole was maintained by the tidal current filling and emptying the extensive estuary to the north-east; but when the training-walls were extended, the tidal flow would be greatly reduced by the accretions and reclamations, and the channel would silt up.

Some objections had been raised by Mr. Wells Owen to Appendix III, in which he had given a translation of the summing up of Mr. Germain's report, with a short explanatory paragraph at the end, for which alone he was responsible. If Mr. Owen had carefully considered Mr. Germain's figures as they stood, he would have seen that, as was stated in the Author's paragraph, Mr. Germain gave in each line of his Table, not the accretion or erosion of each section separately, but the total accretion or erosion from the Rille lights, at the ends of the training-walls, up to the meridian named in the central column. Accordingly, the last line stated that the total accretion (after deducting the erosion in some of

[1] Minutes of Proceedings Inst. C.E., vol. lxx. p. 1.

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the sections) between the Rille lights and St. Christophe, the extreme limits east and west of Mr. Germain's survey amounted to 40,244,100 cubic yards, a fairly simple and comprehensible statement. He had considered that it would have been clearer to have given the actual accretion or erosion in each division separately, ending up merely with their sum; but this could be obtained from the figures given, which he had not felt at liberty to modify, and he had pointed out in which divisions accretion or erosion had actually occurred. Mr. Owen was misled, by his erroneous reading of the statistics, into supposing that erosion had occurred in five successive sections between 1875 and 1880; it had only occurred, as stated in Appendix III, in the second, third, and fourth divisions, and in the last. The chief erosion had occurred in the second division, amounting to 10,274,000 cubic yards, owing doubtless to a considerable variation in the channel during that period, resulting in a temporary augmentation at that part of the capacity of the estuary without any permanent improvement in the shifting channel, just as Mr. Pember pointed out had occurred in the upper estuary of the Mersey. Mr. Owen had concluded that the training-walls had improved and deepened the channel for 8¾ miles below their extremities, whereas a glance at Plate 4, Figs. 1 and 8, would have shown him how changeable the channel was even a short distance beyond the ends of the training-walls, and that, whilst in 1880 there was a continuous channel to the sea at low-water, a bar had formed across the channel in 1883 within 4 miles of the ends of the walls. Mr. Vernon-Harcourt.

Probably the sand scoured out of the bed of the river, directly after the construction of the training-walls, had settled in another part of the estuary; but Mr. Estignard had made due allowance for this in his calculations of the reduction in capacity, and therefore the figures quoted from his Report gave the excess of the accretion beyond the amount which might be assumed to have come out of the deepened channel. Mr. Abernethy was therefore mistaken in attributing the accretion merely to the deposition of the material scoured from the trained channel; for this displaced material was not only considerably less in quantity than the accretion measured, but had actually been allowed for in the calculation.

He agreed with Mr. Giles in regretting that the river had been trained in so sinuous a course. Only a small modification, however, would have been possible, owing to the proximity of high land on each side; and probably the very unsatisfactory line between Aizier and Tancarville, where two very sharp bends were

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Mr. Vernon-Harcourt.

connected by a straight reach, was adopted to save a second training-wall between Aizier and Quillebeuf. Nevertheless, the bend between Quillebeuf and Tancarville should have been made as gentle as possible, for it increased the difficulty of extending the trained channel towards Havre, and checked the influx of the tide. He did not share Mr. Shoolbred's gloomy anticipations of the sacrifice of Havre. It was curious that Mr. Shoolbred, whilst so strongly insisting on the yearly increasing injury of the existing training-walls to Havre, and the danger of interfering with an estuary, should advocate the scheme of Mr. Lavoinne, who considered that benefit had resulted to Havre from the works, and whose proposed southern training-wall interfered much more with the estuary than his own. It would have been better for Havre if no training-works had been commenced, as then Havre would have been secure from any chance of injury by the great transformations in the Estuary; but Rouen had its claims to consideration, and he believed that it would be possible to complete the works, as he had indicated, without endangering the approach to Havre. Sir James Douglass had stated that the deep water had been trending northwards since 1776; but it was clear from Plate 5, Figs. 1–4, that since 1834 the 5- and 10-metre lines had moved considerably southwards. Sir James Douglass also assumed that the deposit had come down the river; whereas in reality the river was clear, and the deposit came in from the sea, and therefore it was not necessary to narrow the outlet in order to drive the deposit out to sea, as in the case of a sediment-bearing tideless river. Accretion would only occur behind the training-walls, provided the oscillation of the tidal current was unimpeded. Accordingly, he considered that it would be unwise to arrest the silt-bearing tidal current by a sort of groyne across the Ratier Bank, as it would cause a great advance of the beach in front of Trouville, which, though of little consequence as a port, was a flourishing seaside town, and should not be damaged without urgent necessity. Moreover, the southern wall projected by Mr. Lavoinne, by preventing the free ingress of the southern tidal current, and diverging it northwards across the mouth of the river, would produce a conflict of currents at the outlet, leading to deposit of silt. He believed also that the free admission of the southern current would be more valuable in maintaining a central deep than injurious in producing accretion. After the banks became raised behind the extended walls, an equilibrium would eventually be re-established in the estuary, provided the tidal influx was not unduly restricted. The average depth at the outlet

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would doubtless be somewhat reduced, owing to the reduction of tidal capacity in the estuary; but this would be of less importance than in many other estuaries, on account of the ample depth existing there at present. Mr. Vernon-Harcourt.

Correspondence.

Mr. P. CALAND observed that the Seine in so far resembled the Clyde, that on both, the trained lower river ended in an estuary opening into a bay of large superficial extent. The conditions on the Clyde were, however, much more favourable than those on the Seine, as according to the Report of Mr. Deas, of 1873, the last that Mr. Caland had received, the depth of water in the mouth of the lower channel of the Clyde had increased since the erection of the training-works, a result that had not, according to the Author, been attained in the Seine. In a scheme of improvement for the Seine, the interest of both Honfleur and Havre must be considered, which places were on opposite sides of the bay, and close to its mouth. In 1856 Mr. Caland was commissioned by the Netherlands Government to investigate, among other subjects, the condition of the Seine, and his Report, published on the 15th of October, 1856, contained the following paragraph:—"It appears to me that the river-improvement works commenced beyond the Amfard and the Ratier Banks, must be further extended if any useful result is to be obtained." At that time the true nature of the problem did not seem to have been apprehended in France, neither had any general scheme of river-improvement been formulated, and the works were carried out from above downwards (instead of beginning at the sea end and working upwards). The latter course might be explained by the existence of shoals at some distance above the mouth (the Traverse de Villequier and the Flac Bank); but in any case a general scheme for the improvement of the whole river might have been planned. It would then have been apparent that the river was very unduly contracted, and that to provide proper space in the trained-channel it should have been widened at the lower end. That the Estuary of the Seine had become decidedly shallower in late years was not, in his opinion, a matter of surprise. Formerly the sand and mud brought down by the river-flood was deposited in thin layers upon the ground outside the river-bed proper, whence it was carried seawards and dispersed by the waves during periods of storm and high flood-tides; but since the river-bed had been narrowed and deepened, the bulk of the transported material was Mr. Caland.

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Mr. Caland. deposited beyond the end of the trained-channel as a compact mass in deep water, where it was no longer susceptible of being loosened by the action of the waves. On the Clyde, if Mr. Caland was not mistaken, extensive dredging operations had been carried out between the training-walls, which had not only prevented the formation of shoals seawards, but had increased the scouring-power of the lower river, with the results of increasing the depth of water in the bay as well as in the trained-channels. On the Seine, on the other hand, since the completion of the works, the deepening of the channel had been left to nature, that was, to the river itself.

As regarded the Seine, many points seemed to require further explanation. For instance:—Had the velocity of the tidal-current undergone any change since the improvement works had been carried out, and if so, to what extent? How far did the sea-water flow up the river now as compared with former times? What was the reason that the time of high-water and of the turn of the ebb was earlier at Havre than it used to be? That the latter result was due to the deepening of the channel seawards did not appear to be probable; most likely the cause was to be sought in the river itself. Another question of importance was:—Where did the action of the flood cease? In 1856 the tidal-limit was at Poses about thirty-three hours distant above Havre, and it would be matter of the greatest interest to know whether there had not been a change in that limit. Mr. Caland thought there might very well have been. The Author considered the point of principal interest at present to be the improvement of the river between Berville and Havre. As far as he was able to judge without further investigation, the plan proposed by Mr. Lavoinne in 1882 was generally preferable to that of the Author. He would be inclined to carry the left training-line farther out into the river, and to lengthen the harbour of Honfleur, while on the right line he would follow Mr. Lemire's plan and curve, bending it to the eastward of Havre, in order to prevent a possible widening of the stream in front of the latter port, and consequently diminishing the depth at that place.

The width of the mouth of the channel on the Author's plan was far too great, and the funnel-shape necessary in every lower river subject to tidal ebb and flow appeared to him to be better provided for in Mr. Lavoinne's scheme. Whether a widening of the channel above Berville might not result from the works proposed could not be determined without further investigation. True, the difference in width between the proposed mouth and the

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outer ends of the training-walls was very considerable, but it must be borne in mind that before the river works were executed the like difference existed between the mouth and the river at, for instance, Vieux Port and Aizier, which places, having regard to the entire length of the lower river, were not so very much higher up than Berville. Mr. Caland.

Mr. Caméré wished to acknowledge the value of the Paper, which constituted a complete and interesting monograph of the basin of the Seine. He would limit his remarks to one or two questions treated of in the Paper, and which he hoped to render more clear. The Author, at p. 230, expressed the opinion that, "the variety of types adopted within one river-basin has been due to the desire of the engineers in charge of the different sections to devise some new and improved type, and not to the special adaptability of each type to the localities in which they are situated." As the best means of combating this opinion, Mr. Caméré proposed to give a succinct history of these different types of weir. The earliest artificial improvement of the rivers of the Seine basin, and which went back several centuries, consisted in passes generally constructed in dams of wood and stone, put down for the use of mills and factories. These openings were closed by beams or needles. To these primitive passes, difficult to manœuvre, and occasioning considerable waste of water, succeeded (notably on the Yonne towards the end of the last and the beginning of this century) passes with side-walls and aprons of masonry 26 feet wide, closed by needles bearing at the top against pivoting-bars. Later on the width of the passes established on the River Yonne was increased to 40 feet, and the supporting bar superseded by a cable, stretched or slacked, as needed. The danger and difficulty to the barges in passing through these narrow openings, naturally led the engineers to widen them, and it was this that led Mr. Poirée, in 1834, to increase the width for the Basseville pass to 72 feet, and to support the needles against frames spaced at intervals between the side-walls. Such was the inception of the type of weir which bore the name of the engineer in question, and which, in combination with the employment of sluices, rendered the navigation of the Yonne and of the Lower Seine incomparably superior to its previous state. Notwithstanding its good qualities, due to the fewness of parts and the simplicity of its manœuvring, the Poirée dam manifested the following faults when proposed to be applied for greater heights, either for diminishing the number of reaches, or for increasing the depth of water:—the obligation of applying needles and frames of unmanageable size and weight; the necessity of Mr. Caméré.

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Mr. Caméré. combining, with dams of this system, overfalls of considerable extent, and therefore costly: the want of having recourse to incessant manipulation, in order to prevent the service-footway from being submerged, the latter rendered probable by the small height above the pool, manœuvres that might even become impossible on the occurrence of a sudden and brisk rise of the river. The impossibility of keeping the needles sufficiently watertight to maintain the normal depth above the weir, by reason of the flow of the river becoming insignificant. It was to meet these disadvantages that the authorities adopted the Chanoine system for the weirs to be constructed on the Upper Seine in 1860. Nevertheless, experience soon showed that the service of the Chanoine weirs presented serious difficulties, and that they were liable to fall down unexpectedly, thus entailing, by reason of the sudden alteration of depth, grave inconvenience to the navigation. As regarded Mr. Desfontaine's drum-weirs, they did not properly constitute a complete system applicable to navigable passes, but rather allowed of the formation of an overfall of extent sufficient, having regard to the ease of manipulation, to regulate the depth without having recourse to the incessant opening and closing of the deeper weirs, and even to afford valuable assistance in the manœuvring of the latter. In this way drum-weirs had been applied between 1857 and 1867 to the overfalls of the Marne weirs, in which the deep passes were closed by Chanoine shutters. The end kept in view by the authorities in installing on the Upper and Lower Seine, as well as upon its principal affluents, dams of the Poirée or of the Chanoine types, was to give to these rivers a depth of 5 feet 3 inches. In so far as the Lower Seine was concerned, it was soon found that this depth, which was being achieved under the law of the 31st of May, 1846, no longer responded to the growing requirements of the navigation, and in 1861 it was decided to put up new dams of the Poirée type between Paris and Rouen. The inquiries undertaken by Messrs. Belgrand, Krantz, and de Lagrené, having led those engineers to think that increased expenditure necessitated by substituting a depth of 10½ feet for one of 6 feet 6 inches would not be excessive, and the Admiralty, to whom were entrusted the preliminary studies relative to these proposals, having decisively pronounced in favour of the greater depth, the latter was adopted by the law of the 6th of April, 1878. To avoid multiplying beyond reason the number of pools, it was necessary, in order to obtain a depth of 10½ feet, to employ dams of a minimum height of 13 feet. Although the Poirée system had never yet been employed for such heights, it was adopted by the authorities in default of any systems

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of well-ascertained superiority. It was, furthermore, the type of dam prescribed in 1876 for application at Port Villez. Being entrusted with the construction of this weir, he had early come to the conclusion that this enlarged type of Poirée dam went beyond the limits of regular working, not only by reason of the damage that it was to be feared might happen, when being lowered, to frames 18 feet high and weighing nearly 2 tons, but also on account of the difficulty to be anticipated in handling the needles, which were proposed to be nearly 8 inches square.[1] These uncertainties so impressed him that, having to design several other dams for the Seine, he hesitated to adopt a type in which he had so little confidence.[2] Mr. Caméré.

Such were the reasons leading him to seek for a type of weir in which all the shutting parts could, in times of flood, be raised above water-level, and in which the obstruction was compatible with deep water in the pool above, a system having the weir at Poses as a first example, and which was described in the Paper (p. 234). At Poses the fall was not less than 21 feet, and the pool constituted by this dam was 26 miles long. The dam was opened on the 23rd of September, 1885, and had stood the test of the exceptional floods of the Seine in the winter of 1885–86. Its working had left nothing to be desired, and it had proved one thing that deserved to be recorded, namely, that no system of weir with parts lowered on the bed of the river could have been capable of satisfactory working under such conditions as had occurred at Poses. As long as it was only a question of constructing weirs of small fall, the lowering was accomplished before the floods had reached the lower pools, and therefore before the water had brought down much detritus. These conditions did not obtain at Poses, where the pent-up water attained almost to the height of great floods. In this case the quantity of *débris* of all descriptions—grass, branches, snags, whole trees brought down and arrested by the dam—was such as would have rendered almost impossible the working of any weir whose main parts were under water, especially as regarded the frames. Notwithstanding

[1] In the first design it was proposed to work these needles mechanically; as carried out they had been replaced by the Caméré hinged curtains.

[2] Since the opening of the Port Villez dam in 1880 numerous breakages and deformations of the frames had occurred, caused by their bedding on stones and stumps brought down by floods and deposited on the sill. It had further been proved that no moving of the frames could be easily accomplished, even with the weir open, when once the water in the lower pool was 1 metre (3 feet 3 inches) below that in the upper one.

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Mr. Caméré. the amount of the obstruction accumulated against the Poses weir before it was raised [1] on the 15th of December, 1885, the opening was effected without difficulty by ordinary means, and without necessitating recourse to the special machinery provided in case of emergency.

The brief history given by him of the Seine weirs would show that the aim had constantly been to keep pace with the demands of the constantly-growing traffic. If engineers still hesitated to pronounce on the rival merits of the Poirée and Chanoine systems as adapted to slight falls, they would nevertheless not doubt that such types were not suited for giving the Lower Seine a depth of 10½ feet, and also they would perhaps come to the conclusion that the Poses weir, in solving this question, had furnished a practical example of a movable dam of great height, and affording every security for working.

The Author's figures, at pp. 233, 235, in his comparison of the cost of the weirs at Poses and Port Villez, needed to be explained, in order that an exact idea might be had of the expense of such structures. At Port Villez, as at Poses, a solid foundation was only found at a great depth, below the beds of gravel and sand forming the river bottom. In order to be beyond the influence of infiltration it was decided to make the Port Villez foundations 24 feet deep; those at Poses were nearly 29 feet below the surface of the sills of the deep passes. These very costly foundations evidently ought not to be considered inherent to the system, and the following figures would give a juster idea of the cost per lineal metre under ordinary conditions, say with foundations 6½ feet deep:—

—	Port Villez.		Poses.	
Extraordinary foundations		£280		£280
Masonry of the sills, piers, &c., including the metallic fittings of the sill . . .	160		£100	
Metallic superstructure, and curtains . .	100		120	
	—	260	—	220
		£540		£500

or £163 7*s*. and £151 5*s*. per lineal foot respectively; but inasmuch as the Port Villez weir only retained a pool 15 miles long, whereas that at Poses retained one 26 miles long, the advantage in respect of result was greatly in favour of Poses.

[1] The amount of this *débris* could be estimated from the photographs exhibited at the Meeting.

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At p. 236 the Author expressed the opinion that, seeing the smallness of the fall designed for Port-Mort, recourse might possibly have been had to a more economical type of dam for that site. Notwithstanding the slight fall (8 feet 9 inches), the height of this weir was not less than 18 feet. Now the inconvenience that had been experienced at Port Villez from the employment of high and heavy frames had naturally led Mr. Caméré to reproduce the Poses type at Port-Mort, of which the good working was assured, and which also presented the advantage of being readily adapted to a greater height should the future requirements of the navigation make it advisable. The footway in the Port-Mort weir, like that at Poses, had been made 3 feet 3 inches above the height of the water upheld, in order to allow later on of a lengthening of the hinged curtains, or of the establishment of small supplementary sluices worked by hand, and so augmenting the height of the water by 20 or even 30 inches, which would easily allow of a depth of 13 feet being maintained in the Seine without the least hindrance to navigation. The facility of raising characteristic weirs of the Poses type was an important advantage which ought evidently to be carried to its credit. Mr. Caméré.

Mr. J. DE COENE presented to the Library, as a contribution to the question under discussion, an abstract of his evidence given before the Technical Commission of the Seine, nominated by the French Minister of Public Works; also a report of the Belgo-Dutch Commission, nominated in 1866 to consider the scheme of a weir for the East Scheldt. Mr. de Coene.

From the former of these documents it would be gathered that Mr. de Coene favoured the narrowing of the outlet of the river, the conservation of a vast interior estuary forming a powerful reservoir; and the construction of a new and completely-sheltered entrance to Havre, of such a character as to give deep-water access to that port at all states of the tide. He desired to draw attention to the views as set forth in the accompanying documents of Mr. Partiot, Inspector-General of Roads and Bridges, who had occupied for several years the position of Engineer of the Maritime Seine. Mr. Partiot had executed part of the estuary works, which had given such splendid results, insomuch that Rouen, formerly accessible only to vessels drawing 10 feet, now received those drawing from 20 to 23 feet, and that for sea-going ships freight and assurance at Rouen were the same as at Havre. The present improved state of affairs gave great value to Mr. Partiot's opinions. Mr. de Coene would remark upon the movement of the alluvial matter brought into the estuary by the flood-

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Mr. de Coene. tide, and afterwards carried away to great depths in the sea by the ebb. The Author's figures were significant on this head. In 1834, before the embankments existed, the capacity of the estuary below the zero of the marine charts was 433,000,000 cubic yards; in 1854 it was 387,000,000 cubic yards; in 1869 (the embankments having been completed in 1860) it was 490,000,000 cubic yards; in 1880, 441,000,000 cubic yards. That was to say, in 1880 with the embankments the estuary was more capacious than in 1834 without them; while in 1869, in spite of the embankments, and contrary to the opinions of the Havre engineers, the estuary was deeper than it had ever been. All rivers with narrow mouths had deep channels, all those with wide mouths had bars. There was no exception. The Mersey formed an example, as did the Seine at present. It seemed therefore to Mr. de Coene that the Paper was not complete as regarded the estuary. The Author had omitted to mention Mr. Partiot's project, which had been made known to him at the Rouen meeting of the French Association, and which offered a solution of the problem of improving the mouths of tidal rivers. Of all examples the most remarkable was that of the Humber, which had a very deep channel of great width, owing to the narrowing of the estuary. That was what engineers should strive to attain. The Author seemed to have relied too much on official information. He saw the subject from one point of view only, and had not treated it with sufficient breadth, but had kept to the beaten track. The comparison of the accretions and erosions of the estuary was a detail, an important one doubtless, but not one suggesting a general law of evolution.

Mr. de Coene ventured also to cite the Belgo-Dutch report. The conclusions arrived at were worthy of remark. It was laid down, for instance—first, that the ebb of a tide carried off more than the flood brought in; secondly, that all efforts should be concentrated in the current of a single channel. Facts supported these conclusions. That was the solution sought in the case of the Seine. In his communication to the Industrial Society of Rouen, he had considered the estuaries of all the great rivers —Gironde, Scheldt, Elbe, Tagus, Foyle, and Tay—and the comparison had led him to a conclusion which had not once been falsified. In a summary study he had lately made of the river Mersey, he expected to arrive at the same conclusion, the realization of which would be a great benefit to Liverpool, where access was impossible at low water.

Mr. Fowler. Mr. JOHN FOWLER (of Stockton) observed that some of the Author's remarks as to the effect produced by the works on the tidal section

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of the Seine seemed to be open to objection. The results which had followed the works on the Seine were akin to what had followed similar works carried out in a corresponding manner in this country. Experience proved that accretion followed training-walls or groynes, whether transverse or longitudinal, the accretion proceeding with more or less rapidity according to the quantity of solid matter contained in the water, and ultimately rising to within a foot or two of the height of the training-works. When the foreshore was narrow and the lateral currents were stopped, the surface assumed a curved slope from the shore, but when wide the surface was level as shown on Plate 4, Fig. 7. The works on the Seine had been carried above average high-water level, and probably reclamation was the primary object in carrying them so high. Chalk was not a good material for the purpose, but by repeated making up it was more likely to stand at one-quarter or one-third tide than at full-tide height. At one part of the Tees where the depth of sand exceeded 40 feet, and the current at unusually high tides was over 4 miles per hour, the wall had been made up four or five times; and whilst the average cost of the walls was 28*s.* 6*d.* per lineal yard this part cost £5 5*s.* The accretion beyond the ends of the training-walls was less than might have been expected, considering the large quantity of soil which had been removed from the channel. Between the training-walls the section on Plate 4, Fig. 2, showed deepening ranging from 10 feet to 36 feet over a length of 28 miles, and an average breadth of ¼ mile; so that taking the average deepening and half width there would be more than 80,000,000 cubic yards of soil removed, some of which would no doubt find a resting place round the ends of the training-walls, but the greater part would be carried downward to the lower estuary. No doubt if the training-walls were of full-tide height to the end, permanent accretion would take place under their shelter. It appeared, however, from the Tables given in Appendix II, that the tidal capacity of the Lower Estuary had been subject to great variation prior to the commencement of the works, and that its condition in 1883 was nearly 11,000,000 cubic yards better than in 1853. It also appeared that between 1875 and 1880 accretion took place at the rate of 11,000,000 cubic yards per annum, whilst between 1880 and 1883 it was under 650,000 cubic yards per annum, and probably it had now ceased. These fluctuations were common to all sandy estuaries, and they often extended over long periods in one direction.

Mr. Fowler.

In 1762 the sandbanks at the mouth of the Tees and the depth (4 feet) on the bar were almost exactly the same as in 1862. Persons unacquainted with the earlier chart jumped to the con-

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Mr. Fowler. clusion that the decrease in depth was due to accretion which had followed the early works of the Tees Navigation Company. No mention was made of any deterioration in the channel beyond the training-walls, and therefore it might be inferred there was none arising from the accretion.

The Author assumed a parallel between the works in the Seine and those proposed for the improvement of the Mersey two years ago. The works proposed in the Mersey, however, differed in three important points from those carried out on the Seine. The training-walls did not terminate in the middle of a sandy estuary but beyond. The training-walls were not proposed to be of full-tide height, but the average height of the sandbanks. The channel was not to be formed by scour but dredged out; instead of 80,000,000 or 100,000,000 cubic yards being driven down to the lower sand-banks, more than 16,000,000 cubic yards of material were to be lifted out of the estuary by dredging. The outer sands at the entrance of the Mersey were entirely separate from the sands in the upper estuary. The narrow channel between Liverpool and Birkenhead gave the direction to the incoming and the outgoing current. Alterations in the form or height of the sandbanks in the bay did not affect the upper estuary, neither did alterations above Liverpool affect the channels in the bay, the tidal capacity remaining the same. The question at issue was, would low training-walls, such as were proposed in the Mersey and had been carried out in the Tees, encourage accretion above their height? The works in the Seine, being so different in character and situation, could give no answer; the works in the Estuary of the Tees showed that they would not.

Mr. Garlick. Mr. E. GARLICK remarked that the training-walls sketched out by the Author in the Estuary below La Rille would, from their convexity, evidently throw the channel from side to side of the new estuary, instead of keeping it always acting in one and the same position, and in a direct course to the sea. At Honfleur the distance between the proposed walls was 1½ mile, and at the mouth of the estuary 5½ miles. In the new wide estuary within the training-walls the channel would wander about without improvement, neither kept to Honfleur nor to Havre. The training-wall 6 miles in length, from Honfleur to Villerville Point, did not exhibit any utility worth the expenditure, for it was parallel with the present shore, apparently exercising no new tidal influence. From an examination of the plan, Plate 4, Fig. 8, he thought the proposal might not injure the navigation down to Honfleur, because there was a possibility of the channel generally remaining

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on that side of the estuary; being then at Honfleur, there was little probability of it making across from there to Havre. But what would be the effect at Havre when the Author's north training-wall was put in, and the Pointe du Hoc deep, which extended up the Estuary for 7 miles, had been stopped up close to Havre by this north training-wall; and when the channel of the Seine had taken, and for a long time kept, a southern course, and all along the north training-wall the reclamation behind the wall was completed, and the accumulation of sand had extended over that wall into the Estuary, as there was every probability from the curvature of the line of wall and wide estuary it would do? Would not the effect on the navigation of Havre be ruinous? Mr. Garlick.

Professor J. GAUDARD had been greatly interested in the question of movable weirs, having recently compiled a notice on this subject for the Bulletin de la Société Vaudoise des Ingénieurs et des Architectes. The systems in use on the Seine suggested the following conclusions:—For weirs frequently raised and lowered a footway was generally adopted; now, from the moment that it existed and could render support to the needles, this simple attachment, sufficient for small heights, supplanted the Chanoine shutters. These shutters were still appropriate for navigable passes, but when their height exceeded 10 feet, it was advisable to replace the winch-barge by a movable footway, and hence arose the question of seeking to use the frames (*fermettes*) as supports for the wall of the dam, and so dispensing with the props and trestles of the Chanoine system. This was what had been done by Messrs. Boulé and Caméré; the needles being found too heavy, these engineers had substituted the more-easily managed curtains, constituting formidable competition with the older shutters. The same idea seemed to have inspired Mr. Pasqueau, in his modification of the Chanoine system on the Rhone. The Tavernier system, with an upper fixed bridge, was more ambitious than those with the movable footways. It had been recommended for the Rhone by Mr. Pasqueau, on account of the stones and gravel brought down by that river impeding the lowering of the frames on the bed. This type of weir, in fact, did not allow of any delicate parts lying on the river-bed. There only needed a sill to support the footings of the uprights of the weir. Now, with the Caméré curtains it would not be difficult to disencumber this sill. It would suffice simply to raise the bottom of the curtains a little, to allow of a sufficient rush of water to clear away any deposit. The Poses weir, opened in October 1885, under the direction of Mr. Caméré, had successfully borne the test of the severe floods of the winter 1885–86. Professor Gaudard.

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Professor Gaudard.

As regarded the estuary of the Seine, the Author submitted a carefully-studied profile for the extension of the embankments, leaving scope for dredging an entrance to Honfleur, which was justified by the power and economy of modern dredgers. Until some time ago the authorities at Honfleur were not above certain modest expedients, sufficient for the time being. They employed osier baskets, anchored by ropes to submerged boulders, in such a way that the flood-tide overturned each basket, and by making it oscillate and by eddying round it, scoured out a hole into which the mooring-boulder sank deeper and deeper. These floating obstructions therefore induced scouring without reefs, as did fixed piles. With eighty baskets, spaced 16 feet apart on a sand-bank, a trench 650 feet long, 4 feet 2 inches deep, and from 16 to 20 feet wide, had been obtained in less than two days, which trench had afterwards been deepened by flushing. This expedient was only successful in currents running from 3 to 4 knots an hour, where, on the other hand, dredging would be impracticable. It had further been proposed to inject air through tubes perforated at the bottom, in order to break up banks where consolidated.

The motion of the tide removed any bar formed of fluvial sediment, not only because it diluted and easily dispersed such material, but because, by mingling with the river-water, it communicated its saltness, and consequently density, to the latter. In a tideless river, on the other hand, it was probable that the heavier sea-water was confined more or less to the lower parts of the estuary. The river-water, in spreading over the surface and losing its forward velocity, allowed the suspended matter to settle. But the example of the Seine showed that, in default of fluvial sediment, there yet remained the sands coming from the sea to be grappled with. The great means of removing these was still the pendulum-like sweeping of the tide, but inasmuch as the tidal broom swept in two directions, it was necessary that the ebb should have greater force than the flood. Velocity was required sufficient to remove sand-banks, without at the same time being dangerous for navigation. In default of a sufficient excess of power of the ebb, the estuary would get choked up by the sand-bearing waters during storms, depositing their burden in places where the cross-action of contending waters destroyed their velocity; and it necessarily required more power to uplift and impart motion to matter once deposited, than to keep it in motion before deposit had occurred. Thus, the Loire between St. Nazaire and Nantes showed a zone where this action and counter-action was indecisive, and where the anchorages filled up in the most persistent manner, albeit good depths were

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maintained in the estuary, which was more contracted than that of the Seine.[1] The preponderating action of the ebb was assured by the addition to it of the fluvial current while the flood lacked that element. By the same token, it would appear that the smaller the volume of the flood, the greater would be this differential action. Nevertheless, it was well known that the volume of oscillating water was of importance for deepening the channel, although there was no occasion to be anxious about the loss of area for expansion by the cutting off of the spaces behind the embankments. The example of the Humber showed that tidal capacity did not suffer from the reclamation of land, so long as a countervailing gain existed in the increased depth of the channel. Now it was known how greatly the Seine embankments had assisted the flood-tide, and allowed it to ascend higher inland. The only fear that need be entertained was, that at the period of low-river, when the latter opposed but little resistance, great storms might act occasionally and partially to choke up the estuary, and that, between whiles, the ordinary movement of the tides might not have sufficient power to restore the normal depths. If that occurred, there remained (were it allowable to think of it) an extreme expedient. It was to induce a torrential reflux—a reverse storm-wave; to produce here what had been done on a smaller scale, yet not without serious difficulty, at the port of Honfleur, namely, to organize a flood by the retention of a spring-tide, temporarily imprisoned by a powerful movable weir, and suddenly released after a certain fall of the tide, navigation being suspended, and banks and other works made strong enough to resist the sudden impulse. It was, however, to be hoped that success would be obtained by less vigorous, and perhaps impracticable measures. On the other hand it was proposed to protect the roadstead by an external embankment, which would stop the accumulation of sand from waves and storms. The idea of scouring by retention and sudden release really meant only the increasing at proper times of the most powerful agent already existing. Floods disencumbered and deepened the channel. The same occurred at the first of the neap-tides, when the river rejected the excess of water which it had accumulated during the period of springs, and the ebb, acquiring more force, deepened the channels seaward. The ebb made the channel; the flood, on the other hand, overturned and equalized such irregularities.

Professor Gaudard.

It was a curious fact that extensive estuaries were found where

[1] Annales des Ponts et Chaussées. 1878. 5e Série. Tome xvi. p. 563.

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Professor Gaudard.

they were of no use. He referred to the glacial rivers, the upper Rhine and Rhone, in the Lakes of Constançe and Geneva. These waters, denser by their low temperature and the sediment they contained than those of the lakes, had a tendency to plunge under the water of the lakes, rather than to disperse themselves over the surface. Recent soundings had shown that, far from causing bars, their action had traced, for a length of several kilometres, profound ravines in the lacustrine depths.[1]

Sir Charles Hartley.

Sir CHARLES HARTLEY, K.C.M.G., having had occasion to study the characteristics of the Seine as a navigable stream, in the preparation of his lecture to the Institution on "Inland Navigations in Europe,"[2] could testify to the accurate description given by the Author, of the chief features of the Seine, and of the results which had followed the efforts made up to this time to improve its condition as a navigable highway. So far as regarded the canalization of the river between Montereau and Paris, he had no remark to offer, save that, from his own observation, the locks and weirs which had been established in that part of the river since 1860, had effectually overcome all the difficulties and delays which formerly attended the tedious system of intermittent navigation, created by artificial floods of necessarily short duration. In considering the improvement of the navigation between Paris and the sea by the valley of the Seine, three separate modes of treatment at once presented themselves to the mind of the hydraulic engineer:—1. The deepening of the bed of the existing water-way by training-works and by dredging; 2. The construction of one or more lateral canals on one side or other of the Seine valley; and 3. The canalization of the river from Paris to Rouen, and thence by a well-trained open channel to the sea. Each of these plans had been attentively studied by French engineers long before the execution of works of any description was decided upon.

The first serious project, after Mr. Cachin's, was that of Mr. de Lamblardie, who, in 1773, proposed to cut a canal on the right bank of the Seine between Havre and Villequier, at an estimated cost of £400,000. This canal was designed to have a length of 31 miles, a width of 121 feet at the water-line, and a depth of 20 feet. Half a century later, Mr. Berigny was charged by the Government to study the numerous projects which had been publicly brought forward up to that period for the improvement of the Lower Seine, and, having done so, he reported in favour of

[1] Bulletin de la Société Vaudoise des Ingénieurs et des Architectes, 1885, p. 53.
[2] The Theory and Practice of Hydro-Mechanics. Session 1884–85. p. 101.

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Mr. de Lamblardie's project. He found it necessary, however, to quadruple the sum originally estimated, owing to the great changes which had taken place along the site of the canal, and to the great advance in wages that had arisen since the first estimate was made.[1] In 1825, a Government Commission, of which Mr. de Prony was the chief technical member, recommended the execution of a grand project, based on new surveys and data of every available kind, for the construction of a maritime canal between Paris and Havre. The length of this canal was designed to be 182 miles (120 miles from Paris to Rouen, and 62 miles from Rouen to Havre); its width, 65 feet at the bottom, and its depth 20 feet. The estimated cost was £8,600,000. In 1830, the execution of this magnificent scheme was arrested owing to revolutionary troubles, and was soon afterwards completely lost sight of; partly because the contemplated expense was so enormous, and partly on account of the rage for the construction of railways, which, spreading throughout France immediately after the first introduction of the locomotive into that country, caused all other public works of magnitude to be neglected for a term of many years' duration. Owing to these circumstances, it was not until 1846, as the Author had related, that energetic steps were taken to improve the Seine below Paris. Since that period to the present day, large sums had been spent annually on works connected with the canalization of the river between Paris and Rouen; but in the lower or tidal part of the river, the expenditure on the embankment-works ceased altogether in 1870.

Sir Charles Hartley.

With regard to the canalization between Paris and Martot, by means of which a permanent depth of 10½ feet was on the eve of being obtained, as contrasted with a depth of only 4 feet forty years ago, he should like if possible, to have it explained by the Author, how it happened that the establishment of a depth of 13½ feet, the intended ultimate depth between Rouen and the St. Aubin lock, near Martot, was not aimed at between Rouen and Paris in the first instance? According to the longitudinal section given by the Author, the cost of obtaining an additional metre in depth in the upper ponds by dredging would have been trifling, in comparison with the immense advantages which would have been gained by the admission of vessels, drawing 13 feet instead of 10 feet, into the very centre of the French capital.

The question of resuscitating the scheme of 1825 for the construction of a ship-canal between Paris and the sea could not be

[1] Navigation Maritime du Havre à Paris. Par C. Berigny. Paris, 1826.

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Sir Charles Hartley.

profitably discussed in the absence of special information relating to the drainage-levels of the Seine valley; and, moreover, he fully agreed with the Author that it would be wise to wait and see whether the greatly improved waterway between Rouen and Paris, now nearly completed, would not suffice to supply all the requirements of Paris, before attempting to burden the State with an enterprize of very doubtful financial advantage.

The Author's account of the training of the river from La Mailleraye (51 miles below Rouen) to Tancarville, La Roque, and Berville, was highly interesting and instructive. The total cost of the work (£1,200,000) including the estimated repairs of the walls, had been small, considering the great benefits the work had conferred on agriculture and commerce. The embankments, although stopping far short of the point designed in 1845 by Mr. Bouniceau, had already accomplished the following results. The minimum navigable depth between Rouen and Havre at high water, at neap tides, had been increased 8 feet, or from 10 feet up to 18 feet, thus allowing the passage of vessels of 2,000 tons at the present time as compared with only 200 tons in 1845. In 1881 the shipping trade of Rouen had increased to 1,472,215 tons, or to treble the tonnage frequenting the port previous to the improvements; and, lastly, 25,000 acres of land had been reclaimed, the value of which was estimated at a much larger sum than the total cost of the works. These substantial advantages, combined with the fact that the bed of the "Thalweg," or centre of the navigable channel, was now lower rather than higher than formerly, were amply sufficient in the opinion of Sir Charles Hartley to justify the embanking of the Seine, even if it stopped short for ever at Berville. He could not therefore agree with the Author that the example of the Seine was a warning to the authorities of other estuaries such as those of the Mersey and the Ribble. It was no doubt unadvisable to terminate training-walls in the middle of a sandy estuary if means could be found to extend them to the sea; but the example of Mr. Bouniceau's uncompleted project alone would serve as a salutary warning if it could be proved that the great improvements of Berville had only been accomplished at the expense of the navigable channel lower down. It was not his intention to refer further either to the Mersey or to the Ribble, being conscious that his ignorance of the principal elements affecting the regimen of the estuaries of those rivers would render any remarks he might offer thereon entirely valueless.

As in all river-works, it would no doubt be possible, judging by

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Sir Charles Hartley.

results, to improve on the plan that had been executed for the improvement of the Seine below La Mailleraye. Apparently, for instance, it would have been better to have constructed the walls with greater solidity, and to have widened out the channel more rapidly as it approached Berville, for in the first case the very onerous cost of maintenance, which had been so much neglected, would have been greatly diminished, and in the second, not only would a greater volume of tidal-water have been admitted, but a much better start could have been made at Berville, as the Author had pointed out, for the prolongation of the walls to the sea. Moreover, the too great contraction of the tidal-space had given rise to undue scour, a circumstance which had abnormally deepened the artificially embanked channel over long distances at the expense of the tidal-capacity of the estuary, although not of its navigable channel. The total amount of scour had been enormous, for, taking the longitudinal section as a basis for calculation, the average lowering of the bed of the trained channel had been about 18 feet. Thus the length of channel being 28 miles from La Piette to Berville, and its average width 440 yards, the amount of silt and sand scoured away and carried into the estuary was 130,000,000 cubic yards, a quantity, it might be remarked in passing, nearly equivalent to, though of course in a great measure independent of, the accretion of 137,000,000 cubic yards, which, according to Mr. Estignard, had been deposited during the period 1866–75, from the end of the training-walls to the meridian of Honfleur. Between the latter and the meridian of Cap de la Hève, according to Mr. Lavoinne, the changes which had taken place during the period 1834–83, had resulted in an accretion of 76,239,220 cubic yards, and in an erosion, principally to the south of the Amfard bank, of 20,772,000 cubic yards; and, in connection with this statement, it should be remembered, although a close comparison of old with recent surveys would lead many experts to form a somewhat different opinion, that both Mr. Lavoinne and Mr. Vauthier (who had carried his investigations for upwards of a mile to the west of Cap de la Hève) agreed in deducing from their Tables, in respect to the outer estuary, that the alterations in capacity by successive accretions and erosions, resulted from variations in the influence of the natural phenomena producing them, and that these changes have no apparent connection with the great alterations which have taken place in the upper estuary.

It ought further to be noticed in connection with the outer estuary, that Cap de la Hève and Pointe du Hoc had always been movable points, the first being continually destroyed by the sea,

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Sir Charles Hartley.

and the second unceasingly ascending the estuary. In short, general opinion, based on personal observation, seemed to be to the effect that transports of alluvion from the river were of small account in causing the deposits in the outer estuary, and that their chief source was from matter brought in suspension from the south, aided somewhat by the erosion of the cliffs to the north, the latter action being demonstrated by the well-ascertained movements of shingle along the shore from north-west to south-east.

With regard to the effect of the works on the tidal flow, Mr. Lavoinne pointed out that the duration of the ebb-tide having been reduced from nine to seven hours above Berville, the velocity of the outflowing current had been considerably augmented, not only between the walls but for a considerable distance below them. He also referred to the advantage of the waters being driven back higher up stream than formerly by the flood-tide, and to the level of low-water at Rouen being about 1 metre lower now than in 1846—results which compensated in a considerable degree, for the great diminution of tidal-volume passing down the estuary. With regard to the present condition of the estuary below Berville, he further remarked that its contracted width, owing to comparatively recent deposits, had also contributed to increase the velocity of the ebb-tide; and that in this way the deposits had played an important part in maintaining and slightly increasing the original depth of the channel. He did not omit to add, however, that the frequent shifting of the navigable channel between the termination of the walls and Honfleur was still a source of difficulty and danger to shipping, especially in foggy weather, and that the condition of this part of the estuary urgently called for speedy redress.

The action of the training-walls in deepening the river between La Mailleraye and Berville (aided by dredging where, as at the Meules bank, the bed of the river was more or less solid), combined with the result of a long series of investigations relative to the regimen of the estuary, induced Mr. Lavoinne to recommend the completion of Mr. Bouniceau's original project as far as the site of the south wall was concerned; to modify the position of his proposed north wall by reconstructing it on a greatly widened out line between Tancarville and Berville; and thence to prolong the north wall on a line gradually widening from 4,000 feet at Berville to 8,000 feet at Honfleur, and to 13,000 feet opposite Havre; as compared with Mr. Bouniceau's 2,000 feet, 4,000 feet, and 7,000 feet at the same points respectively. In Mr. Bouniceau's project it was calculated that the proposed great contraction of the estuary

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between Berville and the sea would have the effect of deepening the channel to 26 feet below zero of the lowest low-tide, by increasing the mean velocity of the current between the walls to 2 knots an hour; but Mr. Lavoinne not only considered it would be impossible to maintain so great a depth, but that the attempt to do so might completely destroy the regimen of the estuary. He was of opinion, moreover, that it would be sufficient for the needs of the navigation if trading vessels were able to ascend to Rouen under the same conditions as at other tidal-ports; in short, that a depth of about 25 feet at high-water of neap-tides, which would only require the lowering of the bed to 4½ feet below zero, would be an ample depth to maintain throughout the channel. With this object in view, he proposed such a width of channel as would ensure a mean velocity of 1½ knot an hour between Tancarville and the meridian of Havre. Sir Charles Hartley would not presume to criticise the project of the late Mr. Lavoinne, who made, it might be said, the improvement of the Seine the study of a lifetime, but he regretted that the late Engineer-in-Chief of the tidal Seine did not consider himself justified in recommending the establishment of a much greater depth than 4½ feet below extreme low-water between the training-walls. For his own part, he considered that a depth of at least 12 feet below zero should be aimed at, a disposition which would involve the establishment of a somewhat narrower channel, and an extension of the pier-heads into deep water; but in his opinion the expense of the additional prolongation of insubmergible walls would be far more than compensated by the greater probability of success, both in an engineering and a commercial point of view, which would attend the operation. Apart from this provision to secure a greater depth, he considered the alignment of the walls to be judiciously planned for securing a stable channel, and for this purpose he could suggest nothing better than the continuously concave southern wall from Berville to the sea. This wall, by completely closing the Villerville channel, which now brought in from the south the greater part of the alluvion deposited in the outer estuary, would moreover ensure the maintenance of a single channel directed towards the north-west in the natural direction taken by the ebb-tide. Again, the entrance was at a spot where erosion was more marked than elsewhere, and where the "Thalweg" was as deep as at any former period. These great advantages would, in his opinion, be lost if the Author's proposed bell-mouth plan below Honfleur were adopted, and he thought therefore that the special but minor advantages, claimed by the

Sir Charles Hartley.

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Sir Charles Hartley.

Author (but which, however, were not admitted by Mr. Lavoinne) in support of his own plan, were no equivalent for its far greater inherent disadvantages as an agent to secure a permanent deep-water entrance to the Seine. Although Mr. Lavoinne advanced many arguments to show that the prolongation of the training-walls would effectually ward off all danger of injurious silting up in the sea approaches to Havre, it was not pretended that otherwise the extension of the embankments would be of any benefit to that port. It was indeed predicted by not a few engineers and mariners, that the completion of the estuary-works would gravely imperil the conservation of the position it now enjoyed of being the second port in France. Nor was it surprising that such fears were entertained, when it was remembered that the draught of vessels trading to Havre was already limited by the depth of water over a wide zone in front of the port, on which there was a depth of only 26½ feet at high-water of neap-tides; the range of tide being 24 feet at springs and 12 feet at neaps, and the depth at the entrance to the harbour being only maintained at 8½ feet below low-water of spring-tides by constant dredging. On the other hand, the fixity of the bed of the sea in the immediate neighbourhood of Havre should beget confidence; for, comparing together all the surveys which had been hitherto made, the unchangeableness of the bed of the roadstead was proved beyond a doubt. This happy circumstance was due, according to Mr. Lavoinne, to the complete absence of matter held in suspension by the alternating tidal currents, and to the independence which existed between the travel of the shingle coming from the north, and that of sand and silt from the south. To this exceptionally favourable condition of things he attributed the immutability of the deep-sea approach to Havre, and he appeared to have had good reasons for concluding that works, such as he proposed for the improvement of the estuary, would not be prejudicial to Havre so long as the double action of the tides and littoral currents continued to prevail. If, however, contrary to Mr. Lavoinne's expectations, further investigations should give good grounds for apprehending the consequences of prolonging the training-walls across the bar into deep water, it would then be more prudent "to let well alone," and to adopt the safest solution of the difficulty, namely, that of digging a deep lateral ship-canal instead of a barge-canal between Havre and Tancarville, and of restricting the extension of the embankment-works to a judicious *raccordement* of the existing walls with the shores of the estuary in the direction of Honfleur.

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Three projects among many others for the improvement of the port of Havre were now being warmly discussed in France— 1st, the project of a large body of experienced naval officers, for the improvement of the existing port by the construction of a breakwater to protect the small roadstead and the existing entrance; 2nd, the project of the Government engineers to suppress the existing entrance (which was directed south-west), and to substitute a new one turned towards the north-west, and the creation of a tenth basin by encroaching on the sea. The expense of these works was estimated at £3,200,000; 3rdly, the project of Mr. Hersent, which embraced three distinct improvements—*a*. The protection of the small roadstead and the existing entrance by a series of isolated breakwaters, beginning at Cap de la Hève, and extending over a length of 4½ miles (including the width of the six openings between the breakwaters) at an average distance of 1 mile from the shore to a point due north of the Ratier bank; *b*. A continuation up-stream of the line of breakwaters by the construction of an insubmergible training-wall to a point 1,600 feet immediately opposite Berville, and of a south training-wall from the latter to Honfleur, where the width of the channel would be increased to 5,000 feet; *c*. The construction of an insubmergible wall round the Ratier bank, in order to ensure the deepening of the two channels, by fixing the position of the north and south currents which separated the channels at the present time. The total estimate for this comprehensive project was £6,706,000, but it was anticipated that of this sum £4,236,000 would be obtained by the sale of reclaimed land behind the training-walls, thus reducing the total sum to £2,470,000. These projects differed so greatly from each other that no better instance could be given of the diversity of opinion that existed in France as to the best means of improving the port of Havre. Moreover, this important question could never be separated from that of improving the estuary of the Seine, when the latter subject was under discussion, and therefore the port of Havre had been somewhat prominently referred to in his remarks on the Author's valuable Paper on the River Seine.

Sir Charles Hartley.

Captain Graham H. Hills observed that the Author had made valuable additions to the facts which in previous Papers he had contributed to the records of the Institution. But in Chapter III. on estuary works he quitted facts to enter the region of speculation, and committed himself to a scheme suggested by the late Mr. Lavoinne, formerly District Engineer for the tidal Seine, stationed at Rouen. The Author stated his objections to some

Captain Hills.

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Captain Hills.

details of Mr. Lavoinne's scheme, but accepted his main argument in favour of a seaward extension of the existing walls, and produced a design showing the modifications of Lavoinne's plan which he would approve. The arguments as put forward by both writers involved important admissions, which might be thus summed up:—

1. The deposits which had formed accretions in the Seine were derived from erosions from neighbouring sea-coasts borne by ocean-currents ranging along those coasts, until traversing the opening of the Seine estuary they were deflected, and their suspended matter was deposited where shelter from prevailing winds was provided by the features of the estuary.

2. Since the commencement of the training-walls in the Seine in 1847 down to 1880, the bulk of the deposits outside of the channel between the training-walls was estimated at 312,000,000 cubic yards; and it was stated by Mr. Lavoinne that eventually the whole spaces now proposed to be cut off from the estuary by the extended training-walls would be filled by similar deposits.

3. It was also admitted that between 1875 and 1880 a large accretion had occurred in the seaward parts of the estuary between the coasts of Havre and Villerville.

On these points he would observe:—

1. As the training-walls were advanced seaward, the conditions of shelter tending to promote deposit were favoured, and as accretions grew the favourable conditions of increased shelter grew also.

2. That the loss of capacity in the tidal-estuary implied by the accretions already recorded, together with that of all future accretions, implied a diminished energy of tidal-stream in the sea-channels, and consequent enfeeblement of the power which tended hitherto to their maintenance.

3. The contention that alternations in the depths of the sea-channels existed before the training-works were commenced, and that losses of capacity similar to those which occurred from 1875 to 1880 used to occur and were subsequently righted by natural causes, was irrelevant, in face of the great changes made in the natural features of the estuary, changes which had altered the effect and operation of natural causes. Assuming, but not granting, that deposits in the sea-channels might have been due mainly to peculiar seasons of storm, the tidal-action being impaired in its out-flow by the tidal abstraction already accomplished, the process which formerly operated to clear out such deposits was proportionately enfeebled; it was therefore mere assumption to maintain that the future could develop such alternations as were said to have occurred in the past. The extension of the training-walls as now

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proposed would leave the estuary-channels to continue hereafter their vagrant tendencies within the limits restricted by the walls; within those limits the elevation of the banks would be promoted by the enfeeblement of the powers which had hitherto resisted or counterbalanced the on-shore drift of storms, and eventually, if the importance of the commerce of the country through Rouen seemed to require it, new training-walls would be established forming a narrow channel sufficient to drain off, and to be maintained by, the tidal and upland water of the river Seine, ending on the sea margin of a delta, such margin falling in with the sweep of the coast-line and the ocean-stream which ranged along it. Captain Hills.

Mr. J. HORAN held that it could hardly be said that the advance made in the science of the treatment of estuaries, with a view to their improvement for navigation, had kept pace with like progress in other departments of modern engineering. Whether this had arisen from the complication of the subject, or that its masters had relied upon individual judgment, followed by tentative work in the few cases successfully treated, without forming or formulating any clear views towards generalization, certain it was that the little knowledge obtainable was to be had only from applying the abstract views of writers on the theory of tides and waves. From the idea of a succession of equal waves in open water, it was only a step to form a conception of the succession of diminishing waves which followed the entry of a tidal-wave into an estuary; diminishing, first, because of the opposition encountered from friction in the channel and other obstacles, and, secondly, from the opposition offered by the descending upland water, and in some cases by a reflected tidal-wave. The diminution effected in this way was not necessarily in all the dimensions of a wave, but related rather to the energy transmitted by a wave to its successor, reckoning from the sea upwards. The phenomena were masked by the great length of the waves as compared with their height, and by interference of the descending upland water when its volume became comparable with the volume of sea-water entering, or with the volume of any of the subsequent transmitted waves. This interference went so far sometimes as to give rise to a bore, just as the mutual opposition of waves produced a chopping sea, or as breakers were formed on a shelving shore, for it must be kept in mind that the oscillation, or to-and-fro movement, produced by a passing wave had its speed lessened on the flow, and increased on the ebb by the descending river. From a consideration of the subject in this light, it followed that there must exist in every tidal estuary, not possessing conditions which Mr. Horan.

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Mr. Horan. might be inferred later on, a state of affairs analogous to what was known as the head of a tide, namely, a region mostly well marked by deposition and an unsettled state of the currents; but owing to the varying relative quantities of tidal and upland waters, this area was not in all cases easily defined. Works undertaken solely for the improvement of the river above this area would tend ultimately to close the estuary, because if such works, by unduly narrowing the channel, increased the velocity of the upland water, the area of deposition would be prolonged towards the estuary mouth, opposition would be offered to the progress of the tidal-wave sooner after its entrance, and the quantity of slack water in the estuary at every tide would be increased as compared with the volume of the sea-water entering. These effects would take place even when the flow of upland water in a given time bore a small ratio to the incoming tidal water in the same time, for scouring power increased rapidly with increase of velocity. A good river could be formed by treatment of this nature at the cost of destruction of tidal-action in its estuary, but clearly the mouth of such a river would always be a problem requiring solution. On the other hand, works to facilitate the passage of the incoming tidal-wave would have the effect of extending the area of deposition up the river, and would also give due scope to the scouring action of the upland water when the tide was on the ebb. Deposition would in this way become distributed over the entire tidal-channel, and in case of upland floods of magnitude, large quantities of silt might be cleared quite out of the estuary and be deposited at sea. This was the maximum result attainable, and hence the soundness of the most successful practice, namely, extending low-water line as far as possible up the river. So viewed, the vexed question of the relative values of tidal and upland waters as scouring agents became narrowed to a question of relative volumes and velocities. The existing works on the Seine could only benefit Rouen at the expense of the ports lower down, and were a warning of the general necessity for united action on behalf of all the interests affected, as against partial treatment of such an estuary. A scheme to preserve a good entry to Havre and Honfleur should include a modification of the channel above Berville, probably to increase its cross-section by dredging, as being the only means now available, since so much of the tidal room had been already lost by the great accumulations at this part of the river.

Mr. Law. Mr. HENRY LAW considered the first division of the Paper as of peculiar interest. The subject of hydrology had received far less attention in Great Britain than it deserved, and unfortunately the systematic study of the hydrology of the river-basins was

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rendered impossible by the irrational manner in which the districts for water-supply, drainage, and river conservancy, were determined. Obviously these districts should be determined by the physical features of the country, and the boundaries should be coterminous with those of the basins, or water-sheds. But as a matter of fact they were determined by the fanciful boundaries of parishes, with the most supreme disregard of the situation of rivers, valleys, and hills. The object of conservancy boards was limited to the preservation and improvement of navigation, their powers were confined to the river alone, and frequently to but a very limited portion of that. If there were a controlling board of conservancy for each large river-basin, then systematic observations could be taken as to all the physical characteristics of each basin, the extent of the several subsidiary valleys, and the distances upon the main stream at which the tributaries from such valleys joined it, gaugings of the various streams under varying conditions, the amount and distribution of the rainfall, the proportions of rain evaporated, absorbed, and delivered from the surface, depending upon the geological characteristics of the soil and of the strata beneath, and the inclination of the surface, and whether bare, cultivated, or covered with wood. One most important matter which influenced the volume of the flow of rivers at different parts of their course was the absorptive, or otherwise nature of their bed; and whether the level of the surface of the river was higher or lower than that of the water contained in the surrounding strata. In some cases it would be found that a portion of the water of the river was absorbed by the bed, and passed into the surrounding strata, of which instances occurred in the Chelt, the Mole, the Stour near Canterbury, and streams at Mimms in Hertfordshire, which latter had no outfall whatever, but were entirely absorbed by the Chalk. In other, and those the most frequent, cases, the river as it pursued its course was fed by springs in its bed, which might be from the superficial strata, or from deeper-seated beds; as an instance, the Thames derived nearly half its flow from spring-water entering through its bed. Observations showed that the water in the Chalk on each side of the valley of the Thames stood at a height of several hundred feet above the river, and as the river was approached, gradually fell until it attained the same level: and much interesting information on this subject would be found in the discussion upon a Paper by the late Mr. P. W. Barlow, M. Inst. C.E.[1] Mr. Law.

[1] Minutes of Proceedings Inst. C.E. vol. xiv. p. 42.

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Mr. Law. It would be difficult to over-estimate the advantages which would result if each large river-basin had its conservancy board with the requisite control over both the land and the water within its district, and which had rating powers by which the means could be provided for making the necessary surveys, conducting the necessary observations, and adopting measures for preventing or mitigating floods, and for improving both the drainage and the navigation. The Author showed how much had been done in the case of the Seine to mitigate floods, by the construction of movable weirs, and by the transmission of warnings to the weir-keepers whenever the upper reaches of the river were in flood. Nothing could be simpler than an automatic apparatus to transmit and record electrically the rise and fall of the river at any point desired, however distant that point might be.

The second division of the Paper was most important, and the details there given as supplemental to the Author's former Paper were of great practical utility. There could be no doubt that the rapid and general development of the railway system in this country had led to the inland navigation being much neglected, and that as a consequence the same attention had not been paid as in other countries, to the improvement of weirs, by means of which while a sufficient depth of water might be retained in dry weather for the purposes of navigation, the rapid discharge of large quantities of water in times of heavy rainfall should be provided for, so as to avoid flooding the adjacent country.

The lessons to be learned from the works, described in the third division of the Paper, which had been executed in the estuary of the Seine, were not only of great importance from the magnitude of those works, from the striking character of the results (so different from those anticipated by the projector), and the rapidity with which those results had been realized; but the subject had special interest from the use made of the experience gained in the treatment of the Seine by the engineers engaged in the great parliamentary fights over the proposed Manchester Ship-Canal.

The arguments, which the opponents to the schemes for the Manchester Ship-Canal founded upon the experience derived from the Seine, might be thus briefly stated: From the peculiar contour of the coast at the mouth of the Mersey, the sea was always endeavouring to close up the entrance to the river, and to form a continuous beach extending from the Lancashire to the Cheshire shores. Were it not for the tidal-flow twice every day of the water entering and leaving the mouth of the Mersey, that mouth would be entirely closed; but the constant passage of this water

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forced an opening for its escape, the dimensions of which passage were dependent upon the quantity having to pass. There were here two antagonistic forces in play, namely, the sea constantly endeavouring to close the mouth, and the passing water as constantly endeavouring to force a passage; and these forces were so nearly balanced that sometimes the one, sometimes the other, predominated; the depth of water and the position of the bar altering from time to time, according as the volume of tidal water, or the force and direction of the prevailing winds varied. It had been proposed in the first two schemes to construct training-walls, to form a definite channel through the sandy inner estuary of the Mersey extending from Garston to Runcorn. The opponents of these schemes appealed to the experience gained from what had occurred upon the Seine (amongst other rivers) in support of their contention that wherever training-walls, were they high or low, were made to form a comparatively narrow channel through a wide sandy estuary, the waters of which were loaded with solid matter in suspension, accretion behind such walls was inevitable. And they went on to say that the effect of such accretion in the Mersey would be to diminish the tidal-capacity of the inner estuary, and as a consequence to lessen the quantity and force of the water entering and leaving over the bar; thus giving a preponderance to the efforts of the sea to increase the height of the bar, until the diminished area of the channel over the same had become adjusted to the diminished flow of water. It was also contended, with apparent reason, that these effects would be cumulative; that, as the banks increased in height, it would become necessary to raise the training-walls, which would again cause a further increase in the banks, until the whole of the inner estuary had been raised to the level of high-water. It must not be supposed, however, that those who adopted this line of argument condemned the use of training-walls. In many situations they were of the greatest value, amongst others where it was desired to canalize a river, and where there was either no bar, or where the training-walls could be carried beyond the bar into deep water.

Mr. Law.

In Mr. Law's experience, increased depth of channel when produced with due regard to the general regime of the river was always attended by increased tidal capacity.

Mr. L. Partiot, having been for nine years in charge of the embankments of the tidal Seine, had had opportunities of carefully studying the question. He had presented various schemes which up to the present time had never been seriously examined; per-

Mr. Partiot.

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Mr. Partiot. haps because they comprised in one project the improvement of the Estuary of the Seine as a whole, entailing a large outlay; perhaps because of the antagonism between the ports of Havre and Rouen, which preferred plans exclusively favouring their several interests. The question having recently been reopened by the French Government, he had taken the opportunity of setting forth his views in a memoir.[1] He had approached the subject from a new side, namely, that of the effects produced by the "narrows," which often separated the estuary of a river from the open sea. It appeared to him quite possible from this point of view to assure a good outfall, while satisfying at once the interests of Havre, of Honfleur, and of Rouen.

He had noticed that when the estuary of a tidal-river, of which the bottom was sandy, was separated from the open sea by a narrow strait, there was produced in the latter a great depth of water, which did not shoal immediately above or below the narrows, but formed channels extending to a considerable distance on either side. He cited in his memoir the outfalls of the Gironde, of the Scheldt, of the Tagus, and of the Mersey, and added that these interior estuaries, which formed in advance of the actual outfalls, remained unchanged for centuries, without being silted up either by the river or by the sea. This rule was equally applicable to bays, such as those of Arcachon and Poole, which only received very small streams. He thus explained this law: When a mass of water, which entered into and retired from a bay passed through a very narrow entrance-channel in a soft bottom, the velocity of the current was augmented, and equilibrium between the bottom-velocity and the resistance of the bed could only be established by a deepening of the latter, varying in extent with the amount of discharge and the width of the channel. The sand in the estuary above was brought down by the ebb through the trench thus produced, and there were formed, by the propagation of this movement, one or several channels, of which the dimensions were dependent on the amount and the duration of the ebb-tide. An analogous cause acted to form, by the flood-tide, a similar channel below the estuary. The sands, thus definitively displaced by the formation of the channels, were deposited in banks above or below the estuary-mouth. In the long extent comprised between the origin of the estuary-channel and its disappearance seaward, the currents did not allow stones, sand, or silt to become stationary.

[1] "Projet des Travaux a faire à l'Embouchure de la Seine. Par L. Partiot, Inspecteur Général des Ponts et Chausées." 4to. Atlas of Plates. Paris 1886.

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They moved up and down, and when the river was of such extent as materially to add by its discharge to the volume of the ebb, they were finally carried out to sea. Mr. Partiot thought that if a channel or gullet, such as that of the Pointe de Grâce, at the outfall of the Seine, were made artificially, there would be produced in the gullet great depths, which would be developed into deep channels, over long distances above and below the new pass. Above there would be formed, between Honfleur and Havre, an extended anchorage. The Author proposed to achieve this result by constructing a mole to above high-water mark, between the Amfard Bank and Villerville, a point situated between Trouville and Honfleur. Only a channel of 6,560 feet wide would be left in front of Havre. Nature appeared to have accomplished by herself a similar operation at the outfall of the River Foyle. The works would be completed by training the Seine between an extension of the embankments as far as Honfleur. This plan would admit of the largest vessels entering Havre at any time; of entering Honfleur at every high-tide; and would allow, even at neaps, of vessels of 800 tons ascending as far as Rouen. Mr. Partiot.

Mr. A. DE PRÉAUDEAU remarked that the hydraulic apparatus at the Bougival lock presented the peculiarity that it was actuated by the lowering of the neighbouring weir. This weir had a sill sufficiently high to assure a permanent drop, and situated, on the left bank, there were hydraulic engines, which served to supply Versailles with water. On the right bank there had been installed, within the body of the weir, two turbines, which worked the pressure-pumps. With the ordinary fall of 6½ to 10 feet, a single turbine sufficed for all the work; but when the fall was reduced by floods to about 2½ feet, the use of both turbines at once might be necessary. The turbines might also be used, should the navigation require it, as sources of power for providing electric-light to the locks. Mr. de Préaudeau.

Between St. Aubin and the mouth, the level of the Seine was constantly affected by tidal oscillation, and important modifications in the behaviour of its waters had been observed since the construction of the embankments. At page 245 of the Paper, it was stated that, "The range of spring-tides, which amounts to 23 feet 7 inches at the mouth of the Seine, is reduced to about 10 feet at La Mailleraye, and to 6 feet 7 inches at Rouen;" in 1861, this amplitude was only 4 feet 7 inches at Rouen. At neap-tides, while there was a depth of 11 feet 5 inches at the mouth, the depth at Rouen had been increased from 2 feet 3 inches in 1861, to 3 feet 7 inches in 1875. In other particulars, the regimen of the river had undergone

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Mr. de Préaudeau.

changes as notable. For navigation, the most favourably situated estuaries were those in which the level of high-water increased regularly and continuously from the mouth to the limit of tidal-action. An example was offered by the river Gironde, which showed that this condition corresponded to great depths near the sea, and to a relatively high rate of discharge of the river. It was not so in respect of the Seine, where the level of high-water springs was, at Quillebeuf, 1 foot 9 inches higher, falling at Duclair to 5½ inches lower, and rising again at Rouen to 8 inches higher than at Havre. Before the embankments were made, high-water rose to about the same level at Rouen as at Havre. The height was less at Quillebeuf, and the depression at Duclair was more sensible. The works having facilitated the action of the tides and of the currents between the embankments, the level of low-water at lowest tides had been decreased between 1840 and 1875 by 2 feet 6 inches at Honfleur, by 4 feet 10 inches at Quillebeuf, by 3 feet 8½ inches at Duclair, and by 4 feet 3 inches at Rouen. From that, it resulted that the difference of low-water levels had varied little between Rouen and Villequier (44 miles), while it had diminished between Villequier and Havre (32 miles). The level of high-water neaps was slightly lowered, it only rose 10 inches higher at Rouen than at Havre, instead of 1 foot 9 inches. It was known that in estuaries as on the seashore, the tides fell lower at springs than at neaps. This did not occur in the tidal part of rivers, when the volume of water sent up by the high-water of spring-tide had not time to pass off so completely between two tides as was the case with neaps. Therefore, the point below which the level of low-water spring-tide was higher than that of neap-tides, defined the limit of the estuary and the beginning of the river. Now this point had changed in the Seine by reason of the works; in 1861 it was 14 miles above Havre; in 1866, 22 miles above, and in 1875, 46 miles above, or midway between Villequier and Quillebeuf.

The essential difference between the profiles proposed by the Author (Plate 4, Fig. 8), and Mr. Lavoinne (Plate 4, Fig. 9), consisted in that the Author left a channel between the Ratier bank and the south shore, which channel Mr. Lavoinne proposed to suppress. Without discussing the details of the lines to be followed between Tancarville and Honfleur, Mr. de Préaudeau thought he was able, from totally different considerations, to support the Author's views. The flood-tides which contributed to the volume of the maritime Seine, appeared to be derived from two principal directions, an ocean-current traversing the Channel from Cape

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Barfleur to Cape Antifer, and a littoral-current which reached the mouth after having traversed the whole of the Calvados coast west and south of the estuary. The ocean-current first reached Havre and the estuary, it afterwards united with the littoral-current, which was thus conducted at an increased velocity to Honfleur. The mingling of these two currents of unequal velocity, and the decrease of the northern current, produced, an hour and a half before high water, a deviation of the flood-tide before Havre, which took a north-north-westerly direction, hugging the shore. The velocity of these currents bearing seaward augmented rapidly, and attained its maximum shortly after high-water at Havre. Now the embankment of the Seine had produced in the estuary a twofold result—diminution of capacity above the mean level by reason of silting, and lowering of the beds in the channels. From the former cause filling took place more quickly than formerly; from the second cause the ocean-current, which arrived first, contributed the more largely to filling. These conditions did not favour access to the port of Havre, and their aggravation should be avoided. It, therefore, appeared that the profile of the embankments should be so disposed as not to facilitate the introduction of the ocean-current, and not to hinder the introduction of the littoral current coming from the south. At present three channels served for the introduction of the tide; they were separated by the Amfard and Ratier banks. Both the schemes under consideration suppressed the channel north of the Amfard, and were so far equivalent; but it could not be doubted that the closing of the channel south of the Ratier would be prejudicial to the introduction of the littoral current. Mr. de Préaudeau, therefore, concurred with the Author, that this channel should be kept open. He would add that, in his opinion, the part of the embankments projected below Honfleur would not be of immediate utility, nor perhaps, even of future advantage. The displacements of the channel had always originated a short distance from the extremities of the embankments at about 6 miles to the east of Honfleur. If the channel were fixed by a prolongation of the embankments, and conveniently widened as far as the meridian of Honfleur, it appeared possible that the combined action of the waves and of the ebb-tide would be sufficient to maintain west of Honfleur the depth necessary for navigation. Therefore, the embankments should be first prolonged as far as Honfleur, and not beyond that point until after the results of sufficient experience had been ascertained.

Mr. de Préaudeau.

Mr. H. S. Ridings thought that greater prominence should be

Mr. Ridings.

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Mr. Ridings. given to cheap (and at the same time effective) methods of controlling rivers which were sometimes applicable. Expensive stone training-walls were in many cases out of the question in poor countries, or where the improvements had to be carried out by small companies or boards of conservators. Also in alluvial districts it often happened that stone could not be obtained, except from a great distance and at heavy cost. His experience in connection with the improvements of the Dique entrance of the Magdalena river, in the United States of Colombia, pointed to the fact that rough logs of timber used as piles (driven as a rule very easily) formed an excellent substitute for stone training-walls. The piles need not be driven close together. There was a sensible diminution of the current behind the piles, and as a consequence a deposition of silt. When the deposit had risen to a certain height, means should be taken to protect the edge next the navigable channel in certain parts, either by close piling, by pitching, or by "mattresses."

As was well known, in many tropical countries, rivers which in the wet season were torrential, in the dry season became small streams. Where land was not valuable embankments might be thrown across valleys, or advantage might be taken of natural depressions, to turn a part of the flood-water into them, thus forming lakes, which in the dry season would become feeders to the main channel. Though this might not be of much service for purposes of inland navigation, it would be of immense service for irrigation. If such a system as this could, for instance, be carried out in connection with the Orange River in South Africa, the advantages would be incalculable.

Mr. Siccama. Mr. H. T. H. SICCAMA remarked that the estuary of the Seine resembled the river Mersey in that it was a river falling into a sea-bay. The true river mouth was at Quillebeuf, up to which point, till the middle of last century, the sea reached. The land reclamation to the west of Quillebeuf dated from 1758, when the Digue Blanche was laid, cutting off the still existing lagoon and surrounding land from the sea. As the volume of water flowing in or out of the river itself could not have sufficient power of scour on the bar, land reclamations in the bay might be a cause of the shallowing between Havre and Honfleur. To cut off more submergible surface would only tend to form a delta lower down, with shifting channels, in a more exposed situation.

Another great point for consideration in estuary-works was the direction in which the tidal-wave approached the coast, as the deepest passes across a bar lay, as a rule, in what might be

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called "scooping on the tide." This might be seen by a glance at the North Sea chart of the Dutch coast for instance. Here the tidal-wave, coming up from the Channel with an average current of 3 miles an hour, approached the estuaries of the Scheldt, the Maas, and the Rhine, and the narrows of Texel and Vlieland, in a direction from the south-west to the north-east, and all the deepest passes across the bars opened up against the south-west. Mr. Siccama.

The great scouring agent in these passes was the current of the rising tide, the velocity of which was not due only to gravity or to the head between high- and low-water, as the current continued to flow for a time at any place after the top of the tidal-wave had passed that place. Nothing should be done tending to check this current by suddenly narrowing the bed or deflecting the direction, as in this case the living force was broken, and only the current due to head was left. The ploughing up of the channels was done by the rising tide, and the sand or silt thus lifted was carried out to sea by the ebb-tide. The unsatisfactory results of the works at the new waterway at the mouth of the Maas was due to losing sight of this fact in devising the intended improvement. This entrance opened at right angles, or nearly so, to the direction of the littoral current, which, therefore, instead of rushing up the estuary in nearly its original direction, swept by, while only a portion of the tidal-wave entered the river mouth. The position of the jetties at the mouth, too, caused great disturbance in the current, which formed continually-shifting pools and tortuous channels, which would not have occurred had their heads been parallel or tangential to its direction. In the Seine, the influence of the littoral current would probably be less than in the case just mentioned, as Havre and Honfleur were situated in a bay; but the tidal-wave coming round Point Barfleur would set up a current which must be of importance for scouring purposes.

Mr. F. G. M. Stoney had devoted much time and attention to weirs and sluices[1] during the last seventeen years, and his experience had confirmed him in the belief that no form of fixed obstruction or uncontrollable weir should be allowed in a river. Whatever form of movable weir was used, it should not shut into the water, nor down on the bed of the river, when open. Above all things, the full capacity of the river should be preserved when the weir or sluices were open, and the sills should be at the bottom of the river, and all should open upwards from the Mr. Stoney.

[1] Minutes of Proceedings Inst. C.E. vol. lx. p. 85, and Plate 6.

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Mr. Stoney. bottom. As an illustration of these principles, Mr. Stoney had erected, some three years ago, a removable weir consisting of four sluices at Belleck, in Ireland. The object was to prevent the flooding of the Erne lakes, which were about 50 miles long of unbroken water, and in which the winter floods rose 9 feet above summer-level. Had the river outlet from the lakes been simply enlarged and deepened, the existing navigation and mill-powers would have been destroyed; but the large sluices were put up to retain water to almost old summer-level, and of such depth and magnitude as to ensure efficient control of any possible flood. These sluices were simply large wrought-iron doors, which shut down tight against planed iron sills set in the bed of the river, and they were lifted vertically from the bottom clear out of and above the running stream at maximum flood.

The great water-pressure against the gates was so transferred to free rollers, that a pressure of 90 tons on each gate was not perceptible, and one man could at any time work the sluices by hand-power. Each gate was 31 feet 6 inches span from centre to centre of the free rollers, 29 feet 2 inches clear span of opening, and 14 feet 6 inches deep. Mr. James Price, Jun., the Resident Engineer of the works, had recently informed him of the working and effect of these sluices after having discharged three winters' floods. Mr. Price had allowed 15 feet 6 inches head of water on the gates, that was 1 foot over the top; but could not feel any difference in pressure on the hand-power over the force required to lift the gates out of the water. The sluices had adjusted themselves perfectly watertight; they never required any repairs. Nor had the least trouble from débris or floating bodies obstructing the rollers been experienced, although there had been a continuous flow up to 700,000 cubic feet per minute through the sluices, often bearing down timber and dirt after the bursting of dams in the works above; everything was carried naturally past, clear of the roller recesses. The whole arrangement of sluices had given complete satisfaction, and the effect in controlling the drainage was all that could be desired. Mr. Stoney added that the additional experience gained from these sluices had more than confirmed all previous expectations as to the working of free rollers on vertical planes, and he saw no practical limit to their advantageous use in very large spans. It was not a question of expediency as to other circumstances and local fitness. As to cost, a movable dam made on this principle would favourably compete with any other structure of like span and depth, while it would be more reliable and efficient. Sluices of this kind

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could also be made to allow a large ship to pass through the sluice-openings in a short space of time. Mr. Stoney.

Mr. L. B. Wells observed that the information given in the Paper on the hydrology of the basin of the Seine naturally directed attention to the principles which obtained in all river basins. In Cheshire the floods in the River Dane, with sources on the steep hill-sides of West Derbyshire and Staffordshire, partook somewhat of a torrential character, while in droughts the river dwindled to very small dimensions; whereas the Weaver, rising in the pervious strata of the low sandstone hills of Cheshire, and having less fall to the sea, was more equable in its flow, and maintained a good supply of water for navigation purposes even during such dry summers as those of 1884 and 1885. The advantage of a system of communication by telegraph, giving warning of heavy rainfall in the district, established upwards of twenty years ago by Mr. E. Leader Williams, when engineer for the Weaver, and of late supplemented by telephonic communication at each weir, had been fully proved. This enabled the sluices to be worked in advance of floods, and the river to be regulated so as to interfere with navigation as little as possible, and yet provide for the passage of the flood-water. It would be interesting to know in what way the floods on the Seine had been influenced by the navigation-works, which must of necessity include the removal of shoals, if not of bends in the river, as well as the construction of locks and weirs. The results might further help to throw additional light upon the old controversy which arose when the Bills for making the Severn navigable were before Parliament, which were revived when the Author's Paper on "Fixed and Movable Weirs" was considered by the Institution in 1880. At that time Mr. Wells mentioned works lately finished and in progress on the Weaver, to provide increased depth for navigable purposes, and to facilitate the passage of floods. Two fixed weirs, by which the water was ponded, were being removed, and eight sluices being erected across the river at Dutton, of 15-feet span, 13 feet deep, with a rise of 9 feet 3 inches at the sluices. Under ordinary circumstances the water flowed over the tops as at the stone weirs, but in floods, by raising the sluices, the river was allowed an uninterrupted flow, and as the sluices were worked from an overhead bridge, there was no danger to attendants, and the necessary work of lifting and lowering could be done effectively, and the river regulated provided the channel below was of sufficient capacity to carry off the flood-water. Following this and minor improvements in the same direction, all tending to the benefit of the navigation, the floods in the Mr. Wells.

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Mr. Wells. Weaver had been brought under better control, and the flood-level lowered, while no interruption to the navigation had occurred through any failure of the sluices. He noticed that the Engineer for the proposed Manchester Ship-Canal, in his Parliamentary plans, had adopted a similar description of weir for maintaining and regulating the waters of the Mersey for navigation purposes. The Author had in 1880 questioned Mr. Wells' view of the trustworthiness of the French movable weirs; but seeing that French engineers were still altering their type, he felt certain there must be some good cause for dissatisfaction. The reports in 1873 by Messrs. W. Forsyth and W. R. Manning, M. Inst. C.E., to the Board of Public Works in Ireland, dealt with the needle-weir, the water-pressure weir, the pontoon-barrage, the drum-weir, and the shutter-weir, each of the systems being in use. The Author stated that shutters had given place to needles and needles to curtains, while now the need was acknowledged of a fixed overhead bridge for working the weir, to which the frames were hinged, not at the bottom of the river, as in the older weirs. The latter type commended itself to him; still the grave objection of a multiplicity of parts remained; these he thought would be liable to derangement, to damage by floating bodies, and to become unworkable during frost. In some ponds on the Weaver, the limit to which the water could be raised had been almost reached, and vessels availed themselves of every additional inch of depth; the water-level had therefore to be closely adjusted, and he had not found any self-acting caps satisfactory. He had tried the cap described by Mr. Baldwin Latham in 1880, and which he had seen working at Nantwich;[1] but there must be a variation of 18 inches in the level of the water to allow a cap 13 inches deep to work, and this exceeded the limit permitted. He had also tried butterfly-valves somewhat similar to those shown in Plate 3, Fig. 8; but found they were not self-acting, and were very liable to foul. He was glad that increased attention was being given to inland water-ways, which, although little considered in this country for half a century, would, he ventured to predict, reassert their worth, and when sufficiently developed prove a great boon to commerce. At present it was next to impossible to obtain goods by canals. They must be freed from railway control, and through routes opened and canals enlarged to a sufficient capacity. It would then be found that for certain classes of traffic they afforded the best means of

[1] Minutes of Proceedings Inst. C.E. vol. lx. p. 95.

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transport. No less than 1,000,000 tons of salt were taken to Liverpool annually by the Weaver, and not a single ton was conveyed by rail. Water-carriage competed successfully with railways for the conveyance of salt to the Tyne, although the cargo had to be trans-shipped from a Weaver barge into a sea-going vessel, and then made the circuit of England, a voyage from point to point of 1,000 miles, whereas the distance by railway was only 170 miles. After the Upper Seine had been deepened from 5½ to 6½ feet, the traffic was stated to have grown 100 per cent., whilst the outlay was not raised more than 50 per cent., and this in France, where the population was not increasing as in this country. He should like to hear the reason for fixing 10 feet 6 inches as the depth of the Lower Seine navigation. He understood the standard canal-depth to be 6 feet 6 inches, as adopted on the Eastern Canal of France and on the Upper Seine. A depth of 10 feet 6 inches was needless in the Lower Seine for vessels trading further inland, but not deep enough to accommodate the ordinary coaster. Coasting steam-vessels were rapidly taking the place of sailing-vessels; these required from 11 to 13 feet depth of water. The Lower Seine could therefore only be used by a class of craft whose voyage must be within the limits of Paris and Rouen, or at the utmost between Paris and Havre. Mr. Wells.

Mr. W. H. WHEELER considered that the system of flood-warnings described by the Author as carried out on the branches of the Seine might usefully be followed in this country, especially on those rivers which passed through low, flat districts. He had frequently brought this matter before the authorities having control over the Fen rivers, but no steps had been taken to put such a plan in practice. He did not gather from the Paper that the training-works in the upper estuary of the Seine had so far caused any detriment to the port of Havre, nor could he see any cause for alarm that such should be the case. On the contrary, it seemed that the channel from the end of the trained-work to the sea had rather improved than deteriorated since the work had been done, the difficulty of navigating this portion of the estuary not arising from the depth of water but from the shifting character of the channel, a defect that had always existed. True, the tidal capacity of the estuary had diminished since the training, but to what extent this was due to the training-works was difficult to determine, as it appeared from Mr. Lavoinne's observations that the capacity of the estuary thirty-two years ago at the lower part was even less than it was now. From the fact that the estuary had deepened below Havre, it would seem as if a portion Mr. Wheeler.

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Mr. Wheeler. of the sand which had oscillated between the upper and lower part of the estuary had now become fixed in the upper part, and the estuary to this extent permanently improved. In this, as in all other similar estuaries, there was a large mass of sand lying about the entrance which was driven in occasionally by storms, to be washed out again by the ebb-current. Although, no doubt, the actual power of the waves and of the tidal-current to move sands was greater than the ebb, yet, taken continuously, the ebb-current must have the greatest effect, as not only had all the tidal-water poured into the estuary and up the channel of the river to return again, but in addition there was the fresh-water from the drainage of a very large area of land, this ebb-current having also the aid of gravity to assist it in carrying the deposit outwards. There was also a large mass of movable sand in the estuary through which the channel appeared to alter its course from time to time, owing to the prevalence of wind from one particular quarter, or to the influence of long-continued floods. The strength of the ebb-current was dissipated in moving these sands and forming new channels. If instead of having to move these sands laterally, the whole energy of the flood and of the ebb-currents were concentrated in one fixed channel, their force would be employed in scouring and deepening it, and thus improving the low-water channel and giving greater navigable depth at high-water. Any loss of scour due to the decrease in the tidal-capacity would then be more than compensated.

For the benefit of the navigation of the lower estuary, it was no doubt desirable to continue the training-works as originally intended, and so fix a deep-water channel in one course, and there would not seem to be any insuperable difficulty in doing this so as not to injure the channels either to Havre or to Honfleur. There was every possibility that they might be deepened and greatly improved. The line of training recommended by the Author seemed open to the objection that owing to the convex form of the proposed north training-wall, the current would pursue a course along the south side and so away from Havre. The line laid down by Mr. Lemire seemed best adapted for serving both Honfleur and Havre. The partially concave line of wall on the north side would probably draw the flood-tide along it, and the ebb would then return along the same course as being the deepest channel. It would be better perhaps if the concavity were continued further on, to opposite Honfleur, and to let the concavity meet the ebb-current nearer the more confined channel. By thus bringing the influence of the ebb-current to bear more directly on

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the channels past Honfleur and Havre, a deep and stable channel would be maintained, and the zone of sands which now existed in the centre of the estuary as a movable mass, would remain fixed in one position. Mr. Wheeler.

Mr. F. WISWALL agreed with the Author that, in point of magnitude, British rivers might not afford opportunities for works comparable with some of those on the Seine; but on the other hand, he believed their capability had yet to be developed, and that they would be found to be at least equal in importance. Mr. Wiswall.

The subject of movable weirs had been under his personal observation for some years in connection with the Mersey and Irwell navigation, on which, within the last three or four years, movable weirs had been introduced of a type entirely novel, and which seemed to possess, but in a higher degree, every advantage attributed to the French weirs. This was so whether in regard to their mobility, or adaptability to the different and hitherto frequently conflicting demands of navigation and drainage, or in their power of discharge at the top with a non-liability to silt at the base; or in their indifference to the action of floods, which, so far from hindering, were pressed into service as an aid to the practical removal of the weir altogether; or in their simplicity of action, and independence of intricate machinery; or in the entire absence of a super-imposed bridge or head-gearing of any kind—a feature which was absolutely unique in the history of movable weirs, supplying in flood a navigable pass over the whole width of the river. As to opening or closing the weir, it need only be said that a weir, 140 feet wide and 10 feet deep, could be either put in or taken out of action in half an hour. The extreme cost of weirs of this type already constructed had not exceeded £50 per lineal foot. The tilting-weir, of which those existing at Throstlenest and Modewheel on the Irwell, near Manchester, and at Warrington, on the Mersey, were examples, was designed to effectually impound the water for navigation, to admit of being either slowly or quickly opened as required, and to give a full and free passage to the river when floods or other circumstances made it desirable. The weir (Figs. 8 and 9) consisted when closed of a series of rectangular sluice-gates turning on a common axis; when opened they were suspended horizontally, and each gate was capable of being worked independently of all the rest. Three edges of the gates made a watertight joint, two by contact with each other, and pressure upon the projecting flanges of the supporting pier, and the third edge by resting upon the floor of the weir. The river-bed was laid with a sheathing of

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Mr. Wiswall. stone and timber, and was perfectly level. At intervals of 10 feet

FIG. 8.

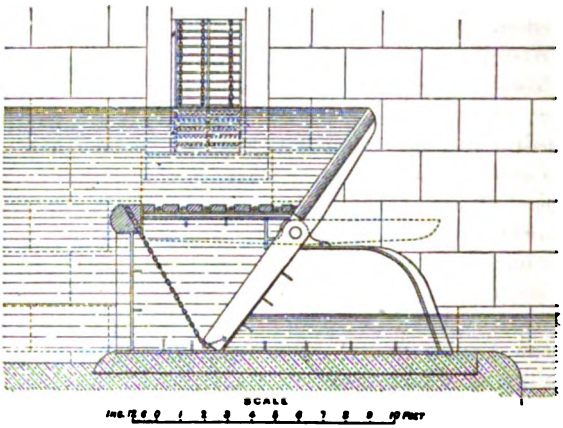
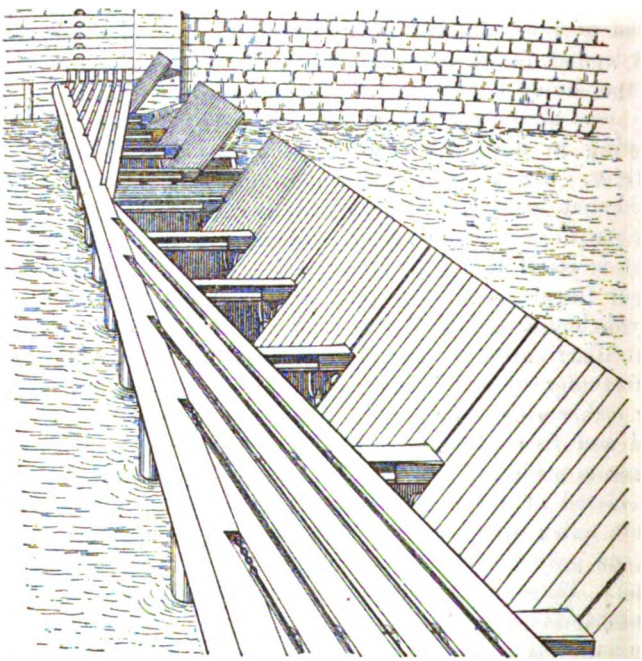


FIG. 9.



from centre to centre, across the bed of the river, cast-iron piers

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were erected, with flanges for securing the piers to the river-bed, and for supporting the tie-beam which extended from shore to shore, and the key-pedestals in which the axle-shafts were set. The turned shafting, on which the gates hung and turned, was in lengths of 10 feet, which reached from pier to pier, terminating in dumb-pedestals keyed to their several seatings on the piers. The gates were framed of English oak, and planked with pine, and were 12 feet long; 10 feet wide above the axle, and 9 feet wide below the axle. A separate draw-chain was attached to the centre of the foot of each gate; it was passed round a grooved pulley secured to the tie-beam, and thence in a straight line to a wall-box, where it was deflected upwards round a grooved pulley to a multiple crab, by which the gate was raised. There were two multiple crabs; one on each side of the river, and each connected with a set of gates. The crabs consisted of sets of three-motion gearing with barrel; that was, one set for each gate-chain. The first motion-shaft, to which the handles were attached, had sliding pinions to enable any one or more of the sets of wheels to be geared to it; the remaining wheels ran loose on the shaft. Rachets were provided to fasten each gate open as it was raised. Mr. Wiswall.

The weir had aroused considerable attention in the neighbourhood, and an indication of opinion was obtained from the fact that the Manchester Corporation had decided to introduce two weirs of this type on the River Medlock, as a corrective to the rapid and disastrous floods to which that stream was peculiarly liable.

Mr. VERNON-HARCOURT, in reply to the correspondence, imagined that the greater depth of 13½ feet, secured at high-water neap-tides between Rouen and St. Aubin, than in the river above, inquired about by Sir Charles Hartley, must be due to a desire to provide in this tidal portion a minimum depth, at most states of the tide, equivalent to the 10½ feet which would be permanently maintained in the non-tidal canalized river up to Paris. The greater depth proposed for the navigation up to Paris, as compared with the normal depth of 6½ feet aimed at for the other French waterways, referred to by Mr. Wells, was, he presumed, intended to facilitate the passage of larger vessels, for the important trade between the sea and the capital of France, than could be economically provided for on the other less important lines of inland navigation. Mr. Vernon-Harcourt.

He thought that answers to Mr. Caland's questions were really contained in the Paper itself. As the tide reached Rouen fifty-eight minutes earlier than formerly (p. 245), it was evident that it flowed up to that extent with greater velocity, owing doubtless to

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Mr. Vernon-Harcourt.

the removal of the shoals which originally impeded its progress. Since, however, the tide rose only an inch or two higher at Rouen than formerly, manifestly the limit of its flow could be but little altered, which might be accounted for by the advantage afforded by the deepening of the bed being counteracted by the undue contraction of the channel. He agreed with Mr. Caland in attributing the changes in the tide at Havre mainly to the changes effected in the Estuary by the training-walls (p. 248). Mr. Caland, whilst expressing a general preference for Mr. Lavoinne's scheme, suggested placing the southern training-wall further out into the Estuary opposite Honfleur, which was in accordance with the plan proposed in Plate 4, Fig. 8. Moreover, though Mr. Caland objected to the rapid widening-out of his proposed training-walls towards the mouth, it was less abrupt at the outlet than Mr. Lavoinne's. There was no necessity to contract the channel opposite Havre in order to deepen it, as there was an ample depth there at the present time; but it was very important to offer no impediment to the influx of the tide, and to restrict the tidal capacity as little as possible.

Mr. Partiot's scheme, proposed originally in 1859, and recently published, should have been included in the list of projects for completing the training-works in the Estuary of the Seine, and he was glad that it had been referred to. Mr. Partiot proposed to prolong the training-walls down to Honfleur, and to build out a breakwater from Villerville Point towards Havre, so as to contract the mouth of the estuary to a width of 6,560 feet alongside Havre. Mr. de Coene appeared to agree with Mr. Partiot in the opinion that a restriction of the mouth of an estuary was the proper method for improving tidal rivers; and they quoted the natural configuration of the Mersey, the Humber, and other rivers in support of their views. He was surprised to hear the Mersey held up as worthy of imitation, for he regarded it as most unsatisfactory, with its shallow channels both above and below the narrows (Fig. 7), and peculiarly difficult to improve. Undoubtedly the narrowing of the mouth of the Estuary of the Seine, proposed by Mr. Partiot, would at first promote scour, and increase the depth in that part of the channel, and for a little distance above and below. This contraction, however, would impede the influx of the flood-tide, and cause changes in the velocity of the current through the narrow neck and in the wide estuary above, promoting the deposit of the silt brought in by the tide. This accretion would be greatly aided by the prolongation of the training-walls to Honfleur, so that eventually the greater

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portion of the estuary comprised between Tancarville, Hoc Point, and Honfleur, would be raised to high-water level. This large reduction in tidal capacity would reduce the tidal current through the narrowed entrance, and consequently diminish again the depth in the channel. Moreover, this reduction of tidal flow in and out of the Lower Estuary would favour the natural heaping-up action of the sea on the sands outside; so that eventually, not only would the initial deepening of the narrowed outlet be lost, but the good depths in the bay outside the Estuary would be imperilled. He believed that the wandering of the channel, by periodically stirring up the silt and thus preventing its consolidation and facilitating its removal by the ebb, was the only cause which prevented the silting-up of large embayed estuaries like the Mersey. If the channel were fixed by training-walls, or any other means, the Estuary outside the training-walls would remain undisturbed and gradually silt up, till the interior sluicing-basin relied upon by Mr. Partiot for scouring the outlet channel would be lost. He was therefore of opinion that Mr. Partiot's scheme, by restricting the tidal flow, would be only temporarily advantageous to Havre and Honfleur, and would finally leave the Estuary in a less satisfactory state than at the present time. Mr. Vernon-Harcourt.

It had been suggested by Sir Charles Hartley that the prolonged training-walls should be placed closer together and carried further out than proposed by Mr. Lavoinne, to secure a greater depth; but this would be treating the Estuary too much like a tideless river, and, by restricting the tidal capacity, would be prejudicial to the maintenance of a good depth in front. He agreed with Sir Charles Hartley that the training of the Seine was fully justified by the results, though the lines and widths adopted might have been modified with advantage; and he only considered that the works served as a warning, from the very unexpected amount of accretion which had so rapidly followed, and especially for estuaries whose maintenance almost wholly depended on tidal scour. Sir Charles Hartley and Mr. Fowler had referred to the considerable scour of sand from the bed of the trained channel as greatly affecting the accretion in the Estuary; but, as he had previously pointed out, Mr. Estignard had made allowance for the increased capacity of the channel, so that the volume of accretion given referred to sediment brought in from the sea, and not to material scoured out of the channel.

Though Captain Graham Hills objected to his drawing conclusions from the facts which he had obtained, a duty incumbent on every engineer, he was glad to find that when Captain Hills

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Mr. Vernon-Harcourt.

entered himself into the region of speculation, his views as to the clearing out of deposit in the Outer Estuary not being equally effected in the future as in the past, owing to the reduction of tidal capacity, agreed exactly with the opinion expressed on p. 249 of the Paper. He could not, however, endorse Captain Hills' assumption of the injurious effects that would result from the proposed extensions, and that a further contraction of the channel would be necessary. He believed that some reduction in depth might occur in the deeper channels beyond the training-works, but that an equilibrium would be restored before any injurious silting-up took place outside; and that the channel between Berville and the outlet must be much improved by the proposed extension.

The Pointe du Hoc deep, referred to by Mr. Garlick, hugged a convex shore at the present time; so that it might reasonably be anticipated that the tidal influx producing it would run close along the northern convex training-wall proposed. The channel, moreover, of the Seine, as recently as 1880 (Plate 4, Fig. 1), did not follow a southern course; and the accumulation of sand had not extended over the existing training-walls as Mr. Garlick suggested. He could not therefore admit Mr. Garlick's conclusion as to the ruinous effect on Havre, based, as he believed it was, on incorrect premises.

The fact that the training-walls below Tancarville were low walls, had been ignored by Mr. Fowler, though it was very plainly stated on p. 244 of the Paper. Mr. Fowler had also assumed, in spite of the evidence on Plate 4, Fig. 1, that the foreshore, behind the training-walls, in the wide estuary was flat, whereas at a short distance from the back of the walls it sloped up rapidly to high-water level. He was also quite mistaken in supposing that reclamation formed any part of the object of the works; and he might see in Mr. Bouniceau's book how little the accretion was anticipated when the scheme was proposed. In drawing any conclusions from Appendix II, it must be remembered that it only related to the portion of the outer estuary beyond Honfleur, some miles below the ends of the training-walls. Mr. Fowler was quite right in noting that it had been intended to dredge out a channel between the proposed training-walls in the Mersey; but he believed that scour would have effected the deepening, and removed the material to another part of the estuary, before the dredgers could have been set to work. As Mr. Fowler said, the question at issue in the Mersey controversy was whether low training-walls would promote accretion beyond their own height; they had

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unquestionably done so on the Seine, and there was an absence of sufficiently exact information to determine what had been the result of the low walls in the Tees, which was the only estuary he knew of where it was contended that no accretion had resulted from training-works. He thought that the exception, if it really existed in the case of the Tees, of which there were serious doubts, should not be accepted as the rule. Mr. Vernon-Harcourt.

The chief criticisms had been directed to the training-walls, which formed the most interesting of the Seine works, because the most uncertain in their results. Though the criticisms differed greatly, he thought that they were valuable as representing every phase of opinion which might be considered in deciding on the question. Wherever the training-walls were extended, fresh accretions must occur behind them; and the difficulty consisted in devising such a scheme that the training-walls should be near enough together to improve the channel down to deep water without unduly restricting the tidal flow. Some engineers objected to training-works as injurious to the tidal capacity of estuaries, which would put a stop to their improvement; whilst others would place them close together to insure a deep fixed channel, which would greatly reduce the tidal flow, and produce a sudden change of velocity of the current at the outlet. The object of his suggested extension was to enforce, rather than to impede, the influx of the tide, whilst regulating and improving the channel in the neutral zone where the influences of the flood and ebb were irregular and deficient.

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23 February, 1886.

Sir FREDERICK J. BRAMWELL, F.R.S., President, in the Chair.

The discussion upon the Paper, "The River Seine," by Mr. L. F. Vernon-Harcourt, occupied the entire evening.

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2 March, 1886.

Sir FREDERICK J. BRAMWELL, F.R.S., President, in the Chair.

The following Associate Members have been transferred to the class of

Members.

HENRY OSBORNE BALDRY.
WILLIAM WORBY BEAUMONT.
JOHN RICHARD BRITTLE, Wh. Sc.
JOHN BREEDON EVERARD.
GEORGE FREDERIC HAMMERSLEY HENNAN.
THOMAS HORN.
LUKE LIVINGSTON MACASSEY.
HENRY MILLER, Jun.

The following Candidates have been admitted as

Students.

ROBERT OGILVIE NEWTON ANDERSON, B.A., B.E.
EDWIN SOUTHEY BEALE.
GEORGE ARTHUR BECKS.
ERNEST HERBERT BURTON.
FRANCIS PHILIP STAINTON CLARKE.
ANDREW DUNCAN.
AMBROSE LEOPOLD EDE.
FREDERICK CHARLES EVERETT.
EDWARD FROBISHER.
CHARLES HENRY GALE.
CHASEMORE PHILIP GATES.
JOHN FREDRICK HARRISON.
HARRY HUNT PEVERIL LE MESURIER.
ALFRED ERNEST LETHBRIDGE.
HARRY MORLEY MILLER.
JAMES ALEXANDER PARKER, B.Sc.
ROBERT SPENCER PERFECT.
FREDERICK WALTER THEODORE SAUNDERS.
THOMAS BOUSTEAD SIMPSON, B.A.
FREDERICK WILLIAM WAREING.
HENRY BOURCHIER WREY.

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The following Candidates were balloted for and duly elected:—

Members.

ROBERT COLLARD.
CHARLES JOHN GALLOWAY.
THOMAS RIDLEY HETHERINGTON.
JOHN DUNLOP PARKER.
THOMAS WILLIAM TRAILL.
ROBERT REEVES TUTHILL, B.A.
HENRY WOODALL.

Associate Members.

JAMES RANSON BAASS, Stud. Inst. C.E.
FREDERICK WILLIAM BRICKNELL, Stud. Inst. C.E.
WALTER HOPE CAMPBELL.
WILLIAM DAWSON.
THOMAS CLARK DEVERELL.
ROBERT FISH.
FULWAR CRAVEN FOWLE.
ALFREDO JOSÉ NABUCO DE ARANJO FREITAS.
JOHN HALLIDAY.
RICHARD JAMES HARTLEY, Stud. Inst. C.E.
WILLIAM ROBERT PAUL HEDERSTEDT, Stud. Inst. C.E.
HENRY CHARLES TERRETT HUNT, Stud. Inst. C.E.
CHARLES EDWIN JONES.
HAROLD JONES.
HARRY HERBERT LAKE.
CHARLES JOHN EDMONDS LARGE, Stud. Inst. C.E.
JAMES MORE, Jun.
JOSIAH PIERCE, Jun., B.A., Stud. Inst. C.E.
CHARLES HENRY PRIESTLEY.
FREDERICK PURDON.
KENNETH REID, Stud. Inst. C.E.
MARTYN NOEL RIDLEY, Stud. Inst. C.E.
STEPHEN PRESCOTT WHITE D'ALTE SELLON.
HARRY ERNEST WALTERS, Stud. Inst. C.E.

The discussion upon the Paper, "The River Seine," by Mr. L. F. Vernon-Harcourt, occupied the entire evening.

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SECT. II.—OTHER SELECTED PAPERS.

(*Paper No. 2133.*)

"Sewer-Ventilation."

By GEORGE RICHARDSON STRACHAN, ASSOC. M. INST. C.E.

THIS Paper contains a record of experiments at Chelsea on sewer-ventilation by down-draughts from cowls.

The experiments are not put forward as novel, but only as accurate observations under known conditions.

The sewer experimented on is in Jubilee Place, Chelsea. It is an egg-shaped brick sewer, 3 feet 9 inches by 2 feet 6 inches, with an inclination of 1 in 100. At the highest part, it terminates in a dead-end, and at the other end joins a main sewer of the metropolitan system. At a point 600 feet from the dead-end a head-wall is built in the sewer, and under it a dip-trap to pass the sewage out. A safety-valve is fixed in the head-wall, to provide a means of discharge in the event of the dip-trap ceasing to act. The sewer is fairly well built, and receives the sewage of forty-four ordinary dwelling-houses, and the road-water from eight gulleys. The mouth of each of these drains is provided with a block-flap. The sewer was thus an elongated vessel, with a capacity of 4,300 cubic feet. The roadway above is practically level. Near the centre of the length of the sewer a 15-inch pipe is inserted at springing-level, and is connected to a shaft against the side of a house. The shaft terminates at the parapet wall, and has on the top a lobster-back cowl, so arranged as to present its mouth to the wind. The mouth of the cowl is 15 inches in diameter, and the throat 9 inches in diameter. The height of the cowl above the road is 32 feet. At the dead-end a 12-inch pipe connects the sewer to a similar shaft against the side of a house opposite. This shaft terminates at a height of 22 feet above the level of the road, and is made to resemble a chimney-stack, surmounted with a 9-inch chimney-pot. At the lower end a 12-inch pipe connects the sewer to a shaft against the front of a house opposite, which terminates at the parapet with an open mouth, at a height of 31 feet above the level of the street.

The action of the system is simple. The wind, in passing the centre shaft, turns the mouth of the cowl towards it, and forces

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air down the shaft and through the 15-inch pipe into the sewer. It then divides into two currents, one passing down the sewer and out by the shaft at the lower end, and the other up the sewer and out by the shaft there.

Four air-meters were used to measure the velocities of the air entering and leaving the sewer. One was placed in the mouth of the downcast pipe in the centre, where it joins the sewer; one in the mouth of the upcast pipe, where it joins the sewer at the lower end; one similarly at the high end, while the remaining one was used as a check-meter at each of these points in rotation. In every case the Author entered the sewer and took the observations.

The following Table shows the results. The experiments commenced on the 26th of February, and terminated on the 11th of April, both in 1885.

Number of Experiment.	Duration of Experiment in Days.	Average Velocity of Wind in Miles per Hour.	Volume of Air entering Sewer in Cubic feet per Minute.	Number of Minutes taken to fill and empty Sewer with Air.	Number of times per Day Sewer was filled with Air.
1	1	18·4	113	38	38
2	3	9·6	126	34	42
3	4	11·3	117	37	39
4	7	11·5	161	27	53
5	7	10·3	136	32	45
6	7	10·6	140	31	46
7	8	11·3	150	29	50
8	7	13·0	180	24	60
8	44	12·0	140	31·5	46·6

The fact that a sewer of this capacity had its gaseous contents changed every thirty-one and a half minutes during forty-four days, by the force of the wind on a self-acting cowl, will probably be considered a satisfactory one. As a matter of fact, one-third of the theoretical volume of air which would have passed through the throat of the cowl at the velocity of the wind, if there had been no resistance or loss by friction, did actually pass through the sewer.

The influence of this volume of air on the interior of the sewer is beneficial. Instead of the slimy coating of bluish-white matter that adheres to the crown and sides of sewers ventilated by openings in the roadway, there is dryness for at least 100 feet on each

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side of the downcast shaft, and then a gradually-increasing dampness is found until the upcast shafts are reached. At these points the crown and sides of the sewer are damp, but the moisture is clean as compared with that of an ordinary sewer.

The air passing out of the upcast shafts has a very slight smell of sewer-gas. No complaints have been made as to it, either by the occupiers of the houses against which the shafts are placed or by the neighbours, or by persons using the street.

The method of forming the shafts deserves mention. They are built against the walls of the houses, and are made of concrete. The pieces are moulded to a rectangular gutter-shape, with a face 12 inches long and sides 6 inches long, so that when placed against the wall which forms the fourth side, a space 12 inches by 6 inches is enclosed. The sides are 1 inch thick. Near the end of each of the short sides a hole is moulded vertically through the piece, so that when the pieces are placed against the wall it exactly coincides with the eyehole of a holdfast driven into the wall. A stout wire is passed through these holes and eyes, and by this method the pieces are threaded to the building. The joints are made good with cement. As the outside of the shaft is marked to correspond to the courses of brickwork, it looks a part of the house, presenting the appearance of a pier. Where necessary, mouldings are placed to further improve it.

It occurs to the Author that the system might be usefully applied by fixing a cowl and shaft, of suitable sizes, at the rear of houses, connected with the house-drains. The air would pass through the house-drain into the sewer, and out of upcast shafts placed at suitable intervals. By this means the house-drains, as well as the sewer, would be ventilated. This method would do away with the necessity of the deep siphons now fixed in house-drains to protect the house from sewer-gas, which the Author has occasionally found, in small property, to become stopped.

The arrangement and design at the Jubilee Place sewer is the work of Mr. Harrington, of Ryde, who claims the idea as his own. On this point the Author has nothing to say. He merely wishes the facts to be recorded for what they are worth.

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(*Paper No. 2137.*)

"On Rail-Joints and Steel Rails."

By CHRISTER PETER SANDBERG, Assoc. M. Inst. C.E.

PART I.—RAIL-JOINTS.

Introduction.

THE reason for this Paper is to be found in the news recently received from America that fish-plates break frequently, and that steel rails wear badly.[1]

The most curious fact is that the angle fish-plates do not break through the bolt-holes, but right in the middle where the rails are joined, and not from the bottom but from the upper edge. This seems to indicate that one of the joint-sleepers must have sunk, so as to put the upper part of the fish in a state of tension instead of compression. As a remedy, some railway administrations have actually increased the length of the angle fish-plates to 44 inches so as to cover three sleepers, of which one is directly under the rail-joint; which means that they have both supported and suspended joints. For more than twenty years rails and rail-joints have been a specialty with the Author in connection with the supervision and inspection of large rail-contracts, for America; as well as of observation of the wear of rails and rail-joints on the railways of Europe. The rails which have been found to wear badly in America are principally of American manufacture, and the fish-plates are also exclusively of American make. This Paper will be confined to a statement of observations in Europe. This may lead to an explanation of the phenomena observed in America, while at the same time it may serve as a warning to European railway-managers to avoid the errors committed elsewhere.

OLD RAIL-JOINTS.

Finding the plain fish-plates too weak, not so much through breakage as through the sinking of the joint and the flattening of the rail-ends, the Author, in 1865, began a great number of experiments in order to ascertain the comparative stiffness of rails and

[1] See numerous articles, reports, and correspondence in "The Railroad Gazette" during the year 1885.

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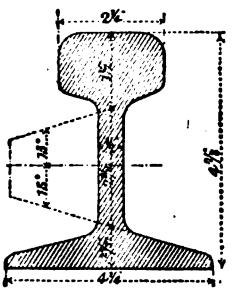
rail-joints. The results were published in the Journal of the Iron and Steel Institute, and in several engineering papers on this side of the Atlantic.[1]

The first suggestion, with a view of strengthening the rail-joint, was to do away with the pear-shaped rail-head in order to obtain a reversible fish-plate of smaller fishing-angle, so as to throw less work upon the bolts, in order that they might not work loose.

In the first series of "Standard rail-sections," published in 1870, this angle was fixed at 22°. But as experience proved that there was some difficulty in rolling rails to this angle, it was increased to 30° in the second series of standard sections for steel rails, published in 1878. The old pear-shaped form had, however, double this angle, or 60°. Several million tons of rails have been rolled to the Author's standard sections, mostly of 50 and 56 lbs. weight per yard; and the largest portion has been sent to America.

Fig. 1.[2]



The next thing was to abolish the notching of steel rails in the flanges, except in the corner of the rail-end, and to drill the bolt-holes instead of punching them.[3]

About this time the use of suspended instead of supported joints was introduced, in consequence of the flattening of the rail-ends. The Author did not advocate this remedy, as is proved by a passage in the pamphlet just referred to, where it was said "It was a poor way to strengthen the weak point of the line (the rail-joint), by taking away the support under it." There was, however, no help at the time, and seeing that the suspended joint would be generally introduced, efforts were directed to securing, if possible, equal stiffness of the joint, as of the solid rail, through the use of stronger fish-plates.

Modern Rail-Joints.

It was soon found that the joint with plain fish-plates would not carry more than say, on an average, one-third of the load carried

[1] Journal of the Iron and Steel Institute, Nov. 1875; and "The Engineer" and "Engineering," Nov. 1876.

[2] The cross-sections are to a scale of ½; the elevations to a scale of ⅛.

[3] "Safety of Permanent Way." By C. P. Sandberg. Printed for private circulation. 1873.

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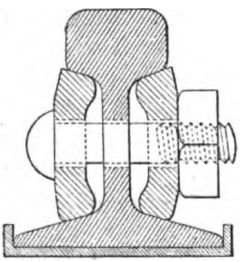
by the solid rail before taking permanent set, the distance of supports being the same as generally obtained. Different shapes of fish-plates were therefore examined, with the view of determining which would give most stiffness, and out of the great variety of shapes a selection of two distinct sorts, the angular and the deep form, was made.

The angular shape projected only to the rail-base, but afforded a much wider base, extending horizontally so that the slot, or holes for the spikes, would fix it to the joint-sleepers, and thus prevent the creeping of the rail longitudinally. The deep fish projected vertically under the rail-base 2 or 3 inches, thus precluding the travel of the rail by the fish butting against the sleepers. In both instances the rail-flanges could be left without any notches.

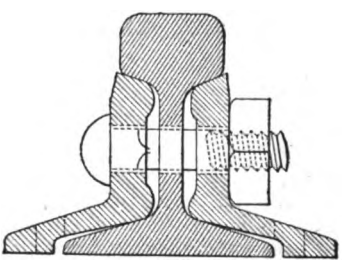
In 1876 the Author suggested that the Administration of the

FIG. 2.



PLAIN FISH-JOINT WITH ELWORTH'S BRIDGE-PLATE.

FIG. 3.



DOUBLE ANGLE FISH-JOINT.

Swedish Government railways should undertake some comparative experiments with these two kinds of fish-plates, on a tolerably large scale, on their lines, so as to ascertain which of the two forms would be the better to adopt. The plain joint then in use, both supported and suspended, had resulted in the flattening of the rail-ends, long before the rails were worn out, so that many thousand tons had to be taken up and reduced in length by cutting off a length of 1 foot at each end, and fresh bolt-holes had to be drilled in order to relay the rails, and get full wear out of them. This proposition was acted upon by the Administration, who not only ordered one thousand pairs of each of the two kinds of fish-plates, but, in addition, got another joint designed by the Chief Engineer of the permanent way, Mr. Elworth. This consisted of an iron bridge-plate, with flanges on both upper sides bearing on the two joint sleepers, something like the Fisher joint

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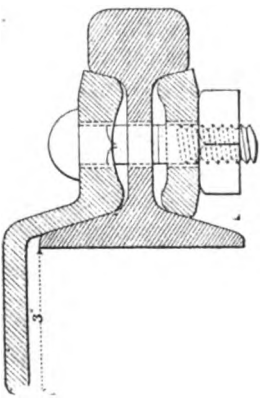
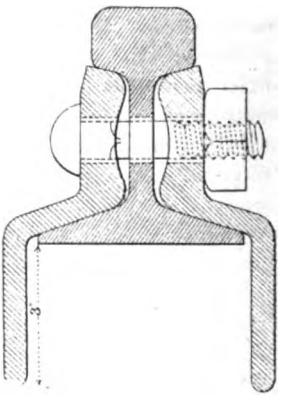
in America, but in conjunction with ordinary plain fish-plates on the sides of the rails.

As the sudden substitution, for plain fish-plates, of either angle or deep fishes on so extensive a system as the Swedish State Railways would have been very expensive, it was proposed to carry out the exchange gradually, by applying at first only one new fish-plate on the outside. With this view different joints were tested, viz., one plain, with one angle, or one deep or one plain on either side,

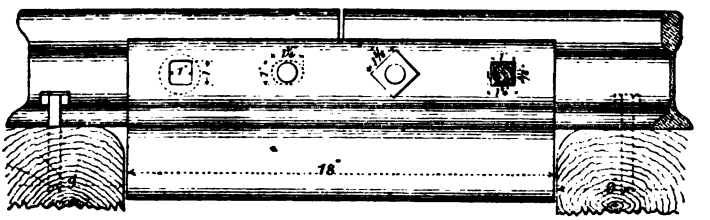
FIGS. 4.



SINGLE DEEP FISH-JOINT.



DOUBLE DEEP FISH-JOINT.



DEEP FISH-JOINT.

and the result is shown in the Table kindly furnished by Mr. Elworth, which is all the more valuable, inasmuch as the information has not hitherto been published. The Author had frequent opportunities of examining the line with these experimental rail-joints, and can testify that each of them had perfectly fair play, there being no pre-disposition for or against any one of them, but only a single aim, namely, to arrive at the best and cheapest rail-joint. The Table shows that the bridge-plate joint (Fig. 2, p. 367) gave the least number of flattened rail-ends; next to that the

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angle fish, and lastly the deep fish-plates; it shows also that the plain, in conjunction with the deep and the angle fish, gave the worst results. Notwithstanding that the bridge-plate joint with ordinary fishes gave the best result, as having less flattened rail-ends and fewer sunken joints, it was considered too expensive for adoption.

The deep fish, although giving the stiffest joint, a joint fully as stiff, indeed, as the rail itself, was somewhat disliked by the platelayers and trackmen, in consequence of the severe Swedish climate; snow and ice covering the line for say half the year.

The angle fish applied on both sides of the rail-joint obtained much favour with the platelayers, but though it became the favourite, yet for economical reasons the exchange has been only gradually effected, one angle fish-plate being applied on the outside, and one plain on the inside. The whole of the Swedish State Railways, about 2,000 miles in length, are, however, now laid with this rail-joint. In future, angle fish-plates are to be applied on both sides of the rail-joint as fast as the plain ones wear out, and the iron rails are changed for steel rails.

RESULTS of EXPERIMENTS with DIFFERENT FISH-PLATES on the SWEDISH STATE LINES.

The following seven different kinds of rail-joints were laid in 1876.

Year when the Rails had to be taken up for flattened ends. Iron Rails 63 lbs. per yard.	Elworth's Bridge-plate or Channel-iron, with plain Fishes.	Angle Fish-plates.			Deep Fish-plates.		
		On both sides of the Rail-joint.	On the inside, with a plain fish on the out-side of the Rail-joint.	On the outside, with a plain fish on the inside of the Rail-joint.	On both sides of the Rail-joint.	On the inside, and a plain fish on the out-side of the Rail-joint.	On the outside, and a plain fish on the inside of the Rail-joint.
	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.
1879	0·8	1·6	3·2	2·4	1·6	1·6	3·2
1880	2·0	4·8	7·2	11·2	11·2	16·0	11·2
1881	3·6	8·0	9·6	16·0	4·8	4·8	8·0
Total of the five years' wear. Rails flattened at the ends in per cent. of rails laid .	6·4	14·4	20·0	29·6	17·6	22·4	22·4

Many of the private lines in Sweden are also laid partly with one angle and one plain fish-plate, and partly with two angle fish-plates, and up to the present time with satisfactory results.

In Denmark, in 1876, the Author's suggestion was adopted on

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the Sjelland State Railway between Corsor and Copenhagen; angle fish-plates were provided on both sides the rail, and the whole line is now laid in this way. Thus in Denmark also, the result of several years' experience has been satisfactory; there has been no breakage of fish-plates, no sunken joints, and no creep of the rails.

In Germany angular fish-plates have long been adopted, both for ordinary wooden sleepers and in connection with the iron or steel sleepers now generally used there; but they differ from the Author's sections. The latter afford contact all along the upper part of the rail-flange (Figs. 5, p. 372), while the German type (Fig. 3, p. 367) only affords equal contact between rail-flange and fish as between rail-head and fish, so there is less probability of bolts keeping tight, although there is equal strain on the bolt on the upper and the lower side. Judging of the comparative merits of these systems from the number of loose rail-joints in the respective countries, the Author claims that the Scandinavian type of joint is decidedly the best.

At the Antwerp Exhibition of 1885 the Author's rail-joint for 50-lb. rails was exhibited, and also the one for 63-lb. rails, made for Denmark, with fishes 26 inches long, and they are favourably spoken of in the Report of the Belgian State Railways. The angle-fishes have thus far shown no signs of failure in Europe, similar to those reported from America; but from their general adoption, say, during the last ten years, they have evidently been regarded by engineers generally as an improvement on the old plain fish-plates. It is true they offer only two-thirds of the stiffness of the solid rail, and the joint sleepers have therefore to be lifted now and then; but the increased stiffness of two-thirds, as compared with one-third of the value formerly attained with plain fishes, is a decided improvement. Again the large base which they offer at the joint sleepers, and the four spikes holding the rails to gauge, are features which cause them to be liked by engineers.

On some lines in Germany the deep fish-plate has been adopted with even better results than the angle, so far as stiffness is concerned, for the line may have with the deep fishes the same stiffness at the joints as with the solid rail.

In France flange-rails are used, but they are ordinarily of heavier weight than in Germany, say 70 to 80 lbs. per yard. Generally plain fish-plates are employed, and joints are laid on the sleepers, but the ballast is good (often being stone), and this is a great help in keeping the joints from sinking,

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In England double-headed or bull-headed rails (not intended to be turned) are mostly used with cast-iron chairs, and deep fish-plates for suspended joints, which suit better with this than with the flange-section. The fish-plates are only 16 to 18 inches long, and weigh 30 to 40 lbs., yet no rail-joints keep better than these; hardly a sunken joint can be detected, and no better or quicker travelling than on the English railways is anywhere to be found. The rails are 80 lbs. per yard or more, and in this great weight lies the secret, which will be referred to under the head of "wear of rails."

The Make of Fish-Plates.

The make of the fish-plates, whether of steel or of iron, is left to the option of the manufacturer. Generally they are punched hot, straightened in a press and the ends filed or cleaned by an emery-wheel from burrs left from the saw. Sometimes, they are cut with shears. For inspection of the angle fish-plates the Author has adopted a block of the same section as the rail, with four dubbs, or projecting pieces, placed as the fish-bolts in the joint. When the fish-plates are fitted into the block for ascertaining the accuracy of the section and bolt-holes, two clapps fixed to the block are turned up with two dubbs, which enter the holes or slots for the spikes to ensure that the spike-holes are in their proper place in relation to the bolt-holes. This is a great improvement, and it only costs the time of an ordinary labourer to put on every fish-plate, and thus secure a perfect fit in the rail-joint.

Of course the square bolt-hole weakens the fish-plate more than the round one; nevertheless in the absence of failure, the Author has of late adopted square bolt-holes in both, and the spike-hole 1 inch from the one end and 2 inches from the other. By this mode both fish-plates are alike, so that makers require only one block for punching bolt and spike-holes, and at the same time engineers do not require to keep the fish-plates in pairs as hitherto, only one kind being needed; still the spike-hole will come 2 inches out of the centre line, so as not to split the sleeper. If the fish-plates are made of steel and punched cold, they must be annealed afterwards in order to regain the strength lost in punching. The drawing (Figs. 5, p. 372) of a complete joint for a light line is of the most modern type, and the same kind is used for heavy lines, but then it is somewhat larger, say, 24 to 28 inches in length. Such is the mode of making and using the angle fish-plates in Europe, and the Author has never heard complaints of their breaking.

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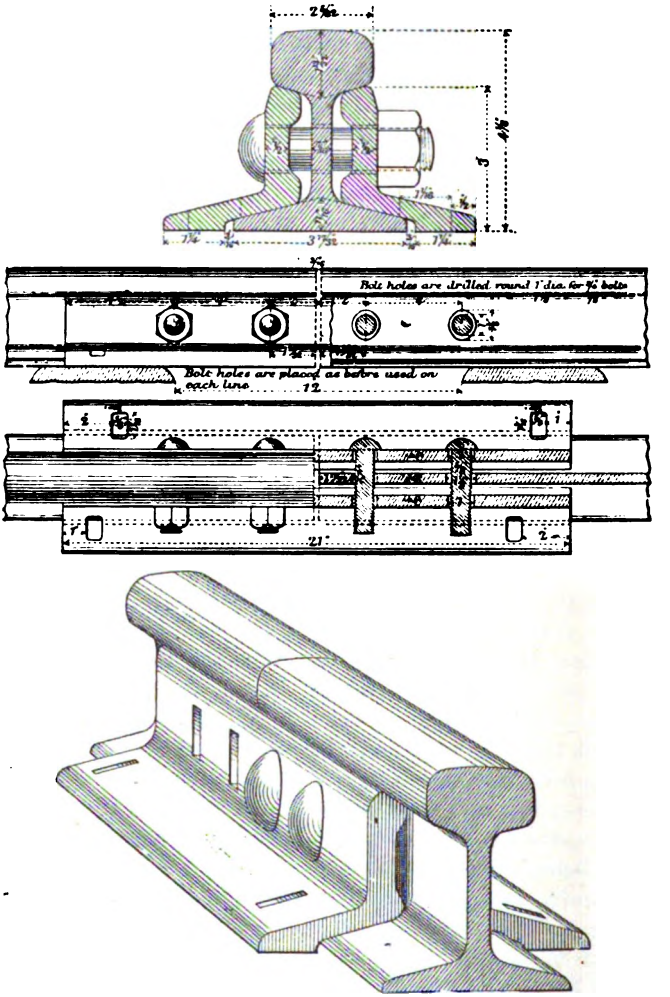
Figs. 6 and 7 (for which the Author is indebted to the courtesy of Mr. John Fritz, of the Bethlehem Steelworks, Pa.) show the form of angle fish-plates on some of the best lines in America.

FIGS. 5.



SUSPENDED RAIL-JOINTS USED IN SWEDEN.

From these figures it may be seen that they are a great deal stronger than those introduced in Sweden, or at any rate there is more metal in them. Another difference between the American

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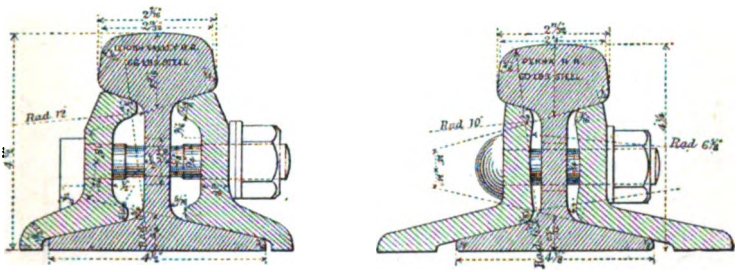
and the Swedish angle fish-plates is that the former have slots while the latter have holes for the spikes. But this is no reason why they should break by thousands on American roads, as stated in the "Railroad Gazette," 1885, where numerous articles have appeared under the heading "Why do Fish-plates break?"

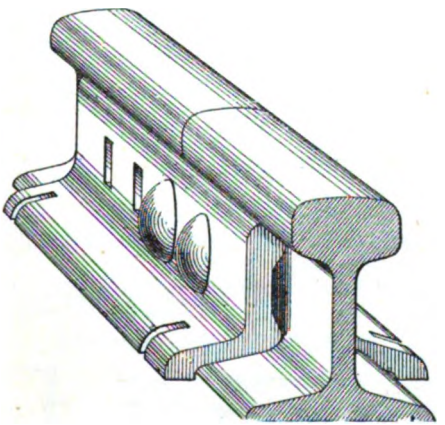
During the last six months many explanations of this most

FIGS. 6.



RAIL-JOINTS USED IN AMERICA.

FIG. 7.



RAIL-JOINTS WITH SLOTS FOR SPIKES USED IN AMERICA.

unfortunate phenomenon have been published in the "Railroad Gazette;" but they have not been altogether satisfactory. Indeed fish-plates are said not to be the right thing for obtaining a perfect line, and bridge-plates, or the so-called "Fisher" rail-joints, are strongly advocated.

In the year 1873, when rails broke through the bolt-holes at the time of punching, and the rails were made much harder than

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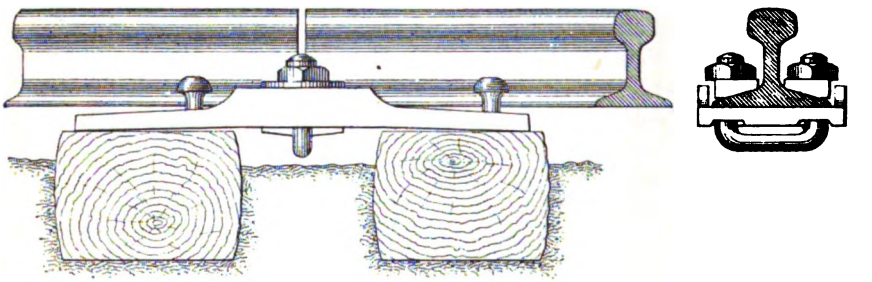
they now are, the Author looked upon the Fisher joint (Figs. 8) as hopeful; but after drilling the bolt-holes had put an end to this kind of fracture the Fisher joint seemed to remain unnoticed, until recently revived in America. Indeed, during the Author's long experience he has only had to inspect one order for rails for the Fisher joint; this was about 15,000 tons for Savannah, in 1882. It need hardly be said that the rail-ends for this joint must be quite square, the section most correct, and the corner notches for the bolt through the rail-flange exact.

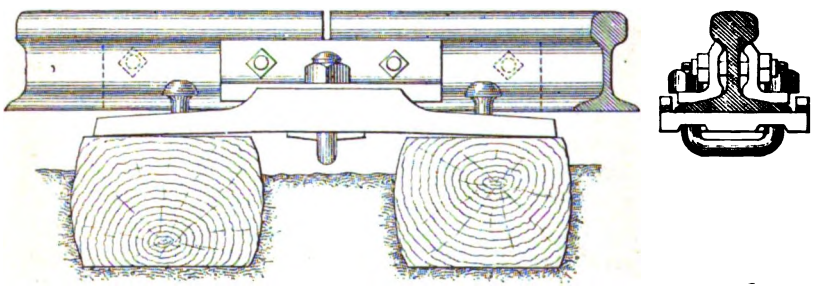
Rail-Joints generally.

To obtain a line of equal strength and stiffness throughout is no easy task, in consequence of the expansion and contraction

Figs. 8.



Fisher Rail-Joint.



Improved Fisher Rail-Joint.

produced by the difference between summer and winter temperature.

To have a joint that can open, say, ½ inch, and shut completely as the temperature changes, and yet be of equal stiffness with the solid rail, is certainly not a simple matter. Even if

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these conditions were fulfilled, the flattening of the extreme ends of the rail-head is almost unavoidable, at least for a weak rail of soft metal. To prove this, a cut say, ½ inch deep, and ¼ inch to ½ inch wide, was made in the head, in the middle of some steel rails, and they were laid down on the Swedish State line. It was found, after a few years' wear, that there were edges flattened on the rail-head, just as at the joints; hence it is the want of support of the molecule just at the end of the rail-head which causes the metal to flow or spread (a kind of "flow of solids," as described by Tresca), and this all the more as the steel of the rail is soft. In order to avoid flattened rail-ends, the corners are sometimes filed off, but there is wanted a hard metal and a big rail-head, of which the end is not crushed nor squeezed out of form. Anyhow with steel rails, it is not so bad as with iron rails, where the imperfect welding caused the ends to be crushed and spread out as a broom. The angle fish-plates having been extensively introduced into Sweden and Denmark, and several other countries, the news of their breaking largely on American railways will naturally cause much anxiety on this side of the Atlantic; and should the same evil be experienced in course of time here, the Author would be the first to look to the Fisher joint or to any other plan to obtain greater safety. He has no particular interest in any joint; yet so far as experience goes, the angular fish-plate has been found much better than the plain fish-plate. In countries with a severe climate, the deep fish-plate was proved to be objectionable; but it has the advantage in mild climates, and particularly in England for double-headed or bull-headed rail-sections.

Part II.—The Wear of Rails.

The second item in the news from America is that steel rails wear badly, so that some are worn out in a very short time. In the spring of 1885 the "Railroad Gazette" contained an article on the wear of steel rails, in which it was stated that the result of wear on some American lines had been rather indifferent, and that a commission had been sent to Europe to inquire whether there was any reason for believing that English rails were superior to those made in America. The conclusion, in short, was that good and bad steel rails could be made, and were made, in Europe as well as in America; and that it rested with the Railway Companies to protect their own interests. The following are the errors indicated:—

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If certain companies bought the cheapest possible rails without choosing a good maker, or if they applied no specification, inspection, or supervision of the manufacture, or did not test the rails, either for wearing-qualities or for safety, they naturally must have taken for granted that all steel rails were equally good or bad, and they had to buy their experience.

If, above all, they overloaded their rails with too heavy rolling stock, or used a 50-lb. rail, of perhaps an indifferent section, where they really should have used a 60-lb. or 70-lb. rail, they made the mistake of trusting too much to the small abrasion of steel as compared with iron, particularly as it had to be made very soft so as not to break.

But any railway company that took proper care of its interest in choosing a good maker, and having a good specification with careful honest inspection, and a sufficiently heavy section, could have rails which gave good results whether they were made in America or in Europe. On the whole the experience in Europe of the wear of steel rails is good; but then the errors noted are generally avoided.

However, one of the leading Railroad Presidents in the United States followed the best of all tests—"experience." He had had excellent experience of the rails he had obtained about eighteen years ago through a certain firm in England, and he did not hesitate to order 10,000 tons of similar rails through the same channel, although it would cost 100,000 dollars more than if the rails were made in America.

Chemical Composition of Rail-Steel.

Through these results of the wear of rails in America the question of the chemical composition of rail-steel comes again to the front. To show that pure steel may not always give satisfactory results in wear, the Author had a few steel rails made of the purest raw materials, for trial on an English railway with heavy traffic, and the result was that they barely came up to the ordinary English steel rail in wear, because they were too soft. They contained about 0·3 per cent. of carbon and hardly any other hardeners. Of course the rail-steel can be made harder, without any risk of breaking, if the material is almost free from phosphorus and silicon. The experience cited above is, however, a proof that even with the purest steel the soft rail does not come up to the hard rail in resistance to wear. This is contrary to what Dr. Dudley, the chemist of the Pennsylvania Railroad, and others have advocated.

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Even in Sweden, which possesses magnificent deposits of pure iron-ores, well adapted to the purpose of steel-making, the manufacture of steel rails has not been largely developed. This is due to the circumstance that Sweden does not possess coal suitable for iron-manufacture; the only fuel used for the purpose is charcoal, and this would make rails so dear that a protective duty of 30 per cent. would be required to make it pay. This duty the State will not grant, but prefers to have cheaper railways, and plenty of them, laid with imported rails. There are now more than 4,000 miles open, which is a higher proportion taken per head of the population than in any other country in the world. It may be added that the Swedish railways have, on the whole, yielded satisfactory financial results; and from a technical point of view, the Author can affirm that no country with a severe climate can show equally good results whether as to wear of rails or to absence of breakages.

A steel with 0·3 per cent. of carbon, where phosphorus and silicon are low, would yield a rail too soft for wear, and the carbon may then well be increased to 0·5. But if the phosphorus reaches 0·1 then it is not safe to exceed 0·3 per cent. of carbon, particularly in cold countries. As to the silicon, so small a proportion as 0·04 per cent., as Dr. Dudley prescribes, is in the Author's opinion a source of blisters and unsound ingots as well as of many wasters in rolling. With 0·1 per cent. of silicon, the steel is more quiet and solid in casting and the rails are equally strong as ascertained by the falling-test, and certainly wear better.

As rail-steel in one district may vary very much in chemical composition from that in another, and still good rails be made in both, it would be unpractical to stipulate in the specification for only one definite composition. Nor is it wise to specify the amount of carbon only, because this should depend upon the amount of the other hardeners present. All that can be said is that rail-steel with carbon as the principal hardener, and with a minimum of phosphorus, is preferable, particularly in cold countries, and that the inspecting engineer should compare mechanical tests with the chemical composition of the rail, and try to find what hardness can safely be used in each individual case. There is no doubt that a rail of medium hardness wears better than one of soft or mild steel, however useful the latter may be for ship-plates and for constructive purposes.

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Tests for Steel Rails.

Why a steel rail should be required to bear a heavier falling-test than an iron rail is a question that makers were in the habit of asking engineers when steel-rail making was begun. The reply was, because of the irregularity of the rail-steel, which was sometimes hard and sometimes soft, as proved by the frequent breaking of steel rails in the road. This was at the commencement of steel-rail manufacture some fifteen years ago, and so the "barbarous" test of a ton falling 30 feet was introduced.

The effect of this was the production of soft rail-steel which would stand such a barbarous test, but the rails thus made now begin to show less durability than the harder rails which were first made and subjected to lighter tests.

During the last ten years, however, makers have greatly increased the probabilities of uniform production. The Author observed this in 1881 both in England and in Germany, when inspecting 100,000 tons of steel blooms for rails to be rolled in America, and a definite chemical composition had to be adhered to. In fact, regularity of production is at present generally obtained except with some new makers, who naturally cannot have the same experience as that acquired by old and long-established firms.

It is astonishing to note the regularity of production of Bessemer steel, and the only hardener which varies according to the heat of the blow is silicon. Bessemer steel is now made for tin-plate works and for ship-plates with an exceedingly small limit of carbon. The time has therefore come when railway engineers may depend upon regularity in the production of rails, and it is almost proved by practice on a large scale, that the best wearing rail is one of medium hardness with say 0·5 per cent. of carbon, provided that phosphorus and silicon are at a minimum. Engineers would do well to reduce the so-called "barbarous" falling-tests to such as would enable makers to produce a somewhat harder rail, but still complying with perfect safety from breakage. The factors causing breakage are high speed, overloading of too heavy rolling-stock, bad roads, and cold climate, and an over-schemed rail-section. With any of these elements of danger in excess it will be necessary to retain high falling-tests, but then a soft rail must be accepted from which a maximum of wear must not be expected. This is, however, no reason why the bulk of rails on ordinary railways under normal conditions should

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not now be made a little harder, so as to wear better, since greater regularity of production can be depended upon. The suggestion is therefore made that the falling-tests should be regulated according to the weight of the rail, as shown in the Table appended to this Paper, always bearing in mind that the rule is applicable only under ordinary circumstances.

The dead-weight tests should remain severe, but no maximum of deflection should be stipulated, beyond that of each rail carrying a certain weight without permanent deflection. Of course, the section has a great deal to do with the results. The higher the rail-section the better it will sustain the dead-weight test, but the sooner it will break under the falling-test.

Over-schemed Rail-Sections.

Some engineers have of late increased the depth of rails, while keeping the same weight, guided by the theoretical formula that the stiffness is in proportion to the square of the height. This would be correct enough if the rail were a girder subjected to permanent dead weight only; but as the rail is subjected to blows or concussions as well as to abrasion, there is a practical limit to the saving of material in this way. Take for instance, an over-schemed rail-section say with half the substance in the head, one-quarter in a thin web of great height, and one-quarter of the substance in a wide and thin flange. With the modern quick-running rail-mills, such a section can well be rolled, but the widely different proportions of the section will cause the flange to come out at a much lower heat than the head as compared with the Author's standard section, in which there is an equal division of material, viz., $\frac{1}{3}$ in the head, $\frac{1}{3}$ in the web, and $\frac{1}{3}$ in the flange. Not only is the latter easier to roll, but it leaves the rolls at a more even heat in the different parts. The former rail-section will have a strain which will always remain in it, through the irregular cooling of the flange and the head; and this all the more as the steel is hard. With an ill-proportioned division of the material, the rails will in cooling draw very crooked, and require a great deal of rough mechanical treatment in cold-straightening under presses, which is often a source of fracture. The Author has generally had to reject no less than 7 per cent. of crooked rails.[1] Thus, the form of the modern over-

[1] "Engineering." 1879. Vol. xxviii. p. 292. "The Straightening of Steel Rails."

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schemed section will in itself be an element of danger, through having a strain in it which will tend to cause breakage with a comparatively light blow when subjected to a heavy engine at high speed, a sunken sleeper, or a cold climate.

As an instance of this undue saving of material, a case may be cited of a railway which, when constructed twenty-five years ago, was laid with 63-lb. iron rails; the weight of the engine was then 27 tons, and the speed comparatively low. Since then the weight of the engine has gradually been increased to 37 tons, while the speed is 50 per cent. higher, and the weight of the carriages has been more than doubled. Nevertheless, 63-lb. rails are still used, but of steel, and of a much deeper section than the original iron rail. In order to guard against breakage of rails in such a case the use of heavy falling-tests and of soft steel is necessitated, but the wearing qualities of the rails must naturally be sacrificed. The introduction of modern rolling-mills has certainly enabled railway-engineers to design more economical rail-sections than in former days, but to go on increasing the height while keeping the weight the same, in order to meet the demand of increased traffic and to produce a stiffer rail, can only result in a source of danger.

The designer of a new rail-section should first consider whether it can be easily rolled; but it often happens that he has never seen a rail rolled: hence the great number of bad sections in the market.

Wearing Results of modern Steel Rails.

A communication to this Institution "On the Manufacture and Wear of Rails," in 1868, led to an animated discussion, and to the conclusion that steel rails, as far as experience had then gone, would last nine times as long as iron rails. It is doubtful whether the experience now at command would bear out this comparison, and for two reasons. First, steel rails are now made softer than they were then; and, secondly, a great deal more work is put on them without any increase in the weight of the rail. If steel rails as now made will endure three times as much wear as iron rails, this is as much as, generally speaking, can be expected from them. One high authority even calculates steel rails to be but slightly more durable than iron rails. The only country whose railways have been modified in accordance with altered circumstances is England.

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The Weight of Steel Rails for Renewals on Roads with Heavy Traffic.

In effecting the change from iron to steel rails, English engineers increased the weight of the rails, say 20 per cent., because they foresaw the heavier work on the road in future, and they got their rails cheaper year by year till the cost of steel was below that of iron rails. Fig. 9 shows the prices per ton f. o. b. shipping port from 1855 to the present time. Continental engineers, on the contrary, reduced the weight of the steel rail 20 per cent. in order to compensate for the higher price of steel

Fig. 9.

Diagram of Average Prices of Iron and Steel Rails since 1855.
The upper line represents Steel, and the lower line Iron.

at the commencement, and since then they have increased the depth of section instead of the weight of the rail. The result is that while in England the principal lines are now completely laid with rails of at least 80 lbs. weight per yard, and have chairs with a base of at least 12 inches in width, Continental lines have mostly preserved their 60-lb. flange-rails, with a base of say 4 inches, and no chairs. How, then, could the results be expected to be the same on the Continent as in England? Allowing, say one quarter of the weight of the flange-rail for the flange,

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instead of chairs as used in England, the railway administrations on the Continent should require a rail of 100 lbs. per yard to correspond to the English 80 lbs., and still the width of base would not be more than half that of the chair. The Metropolitan rail-section weighs 92 lbs. per yard, besides the chairs, but it is so rare to hear of a heavy flange-rail section, that only a short time ago the heaviest flange-rail section in America, of 82 lbs. per yard, was published in the "Railroad Gazette" as a monster.

In instituting a comparison between the cost of English railways and that of a line laid with flange-rails, there must be added, in the former case, the value of the chairs; the value of the rails, sleepers, fish-plates, and bolts being the same in both cases. Taking the weight of each chair at 40 lbs., and the cost of the chair at half that of the steel rails, per ton, and one chair being used per yard of rail, it follows that the cost of 20 lbs. of steel per yard must be added to the cost of the English rail, thus making its value equal to that of a flange-rail of 100 lbs. per yard. Again, comparing the work done on the English railways with that on some of the lines on the Continent and in America laid with flange-rails, it will be found that the average speed is certainly higher in England; but then it must be remembered that there are often steeper gradients on the lines laid with flange-rails, and thus in many localities the speed, though less uniform, becomes equal to that in England. The engines on the Continental lines are frequently heavier than those in England, and the work to which the rails are subjected is therefore often quite as heavy as in this country.

Have, then, English engineers been using unnecessarily heavy rails, or have Continental engineers been laying down rails that are too light? There can be no doubt as to which of the two have been in error, whether regarded from the point of view of safety or of economy. Since the introduction of steel rails in England, derailment has diminished enormously, and the cost of repairs to the permanent way and rolling-stock has much decreased; while the comfort in travelling is greater than in any other country. As the value of time becomes more appreciated by Continental travellers, and as the arrangements for international through traffic—with heavy rolling-stock, such as sleeping- and dining-cars—become more general, the period must soon arrive when the 60-lb. rail will prove too weak, at least on the main lines, and the necessity of replacing it by a stronger rail will then become urgent. This exchange of rails will perhaps take ten years before it can be completely carried out. Meanwhile the

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traffic will go on continually increasing, and also the speed; for statistics show that on some lines the weight of trains has actually been doubled in ten years. If, then, in making the change from the present 60-lb. rail, a 70- or 80-lb. rail is chosen, this will be found, by the time the substitution is completed, to be, like the former rail, much too weak. It will, therefore, in many localities be wisest to take the full step at once, and boldly to adopt a rail of 90- or even 100-lb. section. Of the million tons of the Author's standard sections which have been rolled, most up to the present time have been 50 and 56 lbs. per yard, and but few 60 lbs. The sections of 70 and even 80 lbs., though published with the others as far back as 1870, have never been adopted. If designed on the same basis, the 90-lb. section would be 5½ inches in height and in width of base; while the 100-lb. section should be 6 inches, with an equally strong head and web as that of the English bull-headed rail. These heavy rails should be joined, in countries where the climate is mild, with deep angle fish-plates, and the joints would then be of the same stiffness as the rail in the middle; but in countries with a severe climate they should be joined with angular fish-plates, though the joints would then have only two-thirds of the stiffness of the rails, but the width of base would be increased by say 3 inches, making with the fish-plates 9 inches at the joints.

In suggesting this radical mode of improvement of flange-rail sections, the Author is quite prepared to meet with great opposition from permanent-way engineers; but if they take the safety of the travelling public into consideration (and they will be the first to be blamed when accidents occur), and again if they take the saving in maintenance of road, and particularly in rolling-stock into account—to say nothing of the comfort in travelling—they may gradually come round to these views, and make a change for the better, in order to meet the demands of the future before it is too late.

Metal Sleepers.

It is a great mistake to diminish the rail-section in consequence of the adoption of metal sleepers, as has been done on some railways; for the metal in the sleeper can in no sense make up for the metal in the rail. The strong rail has the power of spreading the effect of the concussions from the rolling-stock over several neighbouring sleepers, thus dividing the effect; while the light, weak rail will concentrate the blows on one or two sleepers. Again, the narrow rail-flange will soon wear off the skin of both rail-flange and

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sleepers; and then fractures will occur, as the metal is not in layers, like the iron made from a pile, but forms one homogeneous mass, in which the least crack may lead to fracture. The heavy flange-rail is therefore a *sine quâ non* for the success of a metallic permanent way; if by "permanency" be meant anything approaching thirty years or more.

In 1880 the United States paid, through their regulation of duties, as much for old worn-out rails as was then paid in England for new steel rails. But even if this should not happen again, the old material will always be equal in value to half the cost of new rails; this, of course, is another reason for not being niggardly in the use of metal when laying down new rails.

It is to be hoped that the iron or steel sleeper now beginning to be introduced, and which may roughly be said to cost as much per ton, and weigh as much per mile of line, as the rails themselves, will enable the Continental roads to rise to the same standing as the English, and also that the adoption of steel sleepers in England (for their introduction is almost a certainty) will prove economical as well as safe.

But if weight is to be saved with sleepers, as it has been with the flange-rails on the Continent, it is rather doubtful what will be the result; and, indeed, the few years of experience with metal sleepers on the Continent show that they have been made too light.

Beyond doubt steel rails and sleepers rust much faster than those made of iron. Why this should be is not quite clear. It may be due to the fact that iron rails and sleepers are rolled at a higher heat than steel, and with floating slag round the pile as a silicate of iron, which is not readily affected by water and atmospheric air; while the steel ingot is rolled at a lower temperature, free from slag, and its pure metallic surface is exposed to the action of air and moisture when put in stack before being laid on the line.

Weight of Rails for New Railways.

In speaking of the weight of rails from the point of view of safety and economy, a distinction should be made between rails for renewals on railways with increasing traffic, high speed, and heavy weight of rolling-stock, and those for railways under construction. In the former case, where statistics may indicate that the traffic will be increased, or even doubled, in ten or twenty years, it is very false economy not to increase the weight of the rail-

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section. It is wise to begin at once, knowing that it may take a long time before the exchange is fully effected, and as a certainty the public will not concede anything, but will require more comfort and greater speed year by year, which will all tell upon the rail.

But for permanent-way in course of first construction the case is different. There, two kinds of road should be recognized, according to local circumstances. First, there is the case of flat countries, with small gradients and easy curves, where an even speed and light traffic are to be expected. For such a case light rails are sufficient, even for standard-gauge lines. In the south of Sweden many such lines are now constructed of the normal gauge, with 30- to 35-lb. rails, at a cost of considerably less than £2,000 per mile, rolling-stock included, and still this section of rail can carry the State-line trucks, and answers all local requirements; but of course the speed is low, the maximum being, perhaps, 15 miles an hour.

But a second case arises in hilly countries, for there it would be false economy to use such a light rail, where the gradients are steep, say 1 in 60. A heavy rail is, indeed, essential for economy of construction of the line, as labour is dear and steel cheap. In this connection, Mr. H. C. Mais, M. Inst. C.E., Chief Engineer of the South Australian railways, says:—"In England it is everywhere conceded, by those conversant with the construction and working of railways, that to lay down light permanent-way in hilly districts where steep gradients must of necessity be used is the reverse of economy, because to work steep grades, heavy engines must be used, and therefore a light permanent-way would be inadmissible; but on level country a light road may be adopted where the traffic is limited, and where there are no steep gradients."[1]

In America the temptation to economize the weight of rails must, however, be immensely greater than it is in Europe, inasmuch as the price of rails is much higher; through 100 per cent. of import duty, the rails were made for years doubly as dear as on this side of the Atlantic, but the cost is now greatly reduced. It therefore seems fair to suggest that the bad results in the wear of steel rails in America, now complained of, may be caused by the use of rails which are too light in proportion to the rolling-stock, the speed, and the traffic.

It is true that the weight of the American engines and carriages

[1] "Report on Observations on Railways." By Henry C. Mais, M. Inst. C.E., Lond. Adelaide, 1884.

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is distributed on more axles than in Europe, and that the bogie is also apt to save the road to some extent; but with a demand for increased speed, the total load of the engines and carriages must tell on the rails, and therefore the duration of a comparatively light rail must naturally be less than that of the heavier rail used in Europe, and particularly in England.

Reduced Tests for Steel Rails.

As regards badly-wearing steel rails, the Author knows of no better suggestion than to use harder steel (which would necessitate reduction of falling-tests), and, at the same time, to use heavier rails. To increase the hardness, while keeping to the same weight of rail, may in many instances prove disastrous, and lead to fracture and accidents. The rail-makers' complaint, that it is the engineers' faults through stipulating too heavy tests and forcing them to make a soft rail, might be true enough, but engineers were quite right to be on the safe side, as long as irregularity in production existed. Now as the time has come when engineers can obtain what they require, it is to be hoped that the change from soft to hard rail, in order to obtain better wearing results, will be effected gradually, and only after very mature consideration of safety before economy.

The factors on the one side are weight of engine, rolling-stock, and speed, local circumstances, and climate; all these belong to the rolling-stock department. On the other side, to meet these demands on the permanent-way, the engineer has to consider perfect safety obtained by providing a good road, a good rail, good rail-section of sufficient substance and weight, medium hardness of rail-steel, and a perfect rail-joint.

Unfortunately, however, these two departments do not always pull together, very much to the loss of the shareholders, as well as to the risk of the travelling public. In executing the changes now suggested, the Author's greatest wish is that perfect union may exist in the two departments, and that the temptation to economize by using a harder rail may not be effected at the sacrifice of safety: in most instances this can only be obtained by a proportionate increase in section and weight of the rail itself. With this view the following Table for testing steel rails is submitted, in the hope that it will be for the benefit of the producer as well as of the consumer.

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PROPOSED TEST for STEEL RAILS.

Lever Test.					Ball Test.					
Weight per Yard.	Distance of Bearings.	Load.	Deflection for pieces, 6 feet long Rails.		Weight per Yard.	Distance of Bearings.	Weight of Ball.	Height of Fall.	Proportion of Test to Weight per Yard.[1]	Deflection usually obtained for pieces 6 feet long Rails.
			Temp.	Perm.						
Lbs.	Feet.	Tons.	Inches.	Inches.	Lbs.	Feet.	Cwt.	Feet.		Inches.
30	3	5	$\frac{1}{8}$	0	30	3	10	6	2·0	2 to 4
		11	$\frac{3}{4}$ to $\frac{3}{4}$	$\frac{1}{8}$ to $\frac{1}{4}$						
35	,,	6	$\frac{1}{8}$	0	35	,,	10	7	2·0	2 ,, 4
		13	$\frac{1}{4}$ to $\frac{3}{4}$	$\frac{1}{8}$ to $\frac{1}{4}$						
40	,,	8	$\frac{1}{8}$	0	40	,,	10	8	2·0	2 ,, 4
		18	$\frac{3}{4}$ to 1	$\frac{1}{8}$ to $\frac{3}{4}$						
45	,,	10	$\frac{1}{8}$	0	45	,,	10	9	2·0	2 ,, 4
		22	$\frac{1}{2}$ to 1	$\frac{1}{8}$ to $\frac{3}{4}$						
50	,,	12	$\frac{1}{8}$	0	50	,,	20	5	2·0	2 ,, 4
		25	$\frac{1}{2}$ to $1\frac{1}{4}$	$\frac{1}{8}$ to 1						
56	,,	14	$\frac{1}{8}$	0	56	,,	20	7	2·5	2 ,, 4
		26	$\frac{3}{4}$ to 2	$\frac{1}{8}$ to $1\frac{3}{4}$						
60	,,	16	$\frac{1}{8}$	0	60	,,	20	9	3·0	2 ,, 4
		32	$\frac{1}{8}$ to $1\frac{3}{4}$	$\frac{1}{8}$ to $1\frac{1}{4}$						
65	,,	18	$\frac{1}{8}$	0	65	,,	20	11	3·3	2 ,, 4
		35	$\frac{1}{2}$ to $1\frac{1}{2}$	$\frac{1}{8}$ to $1\frac{1}{4}$						
70	,,	20	$\frac{1}{8}$	0	70	,,	20	13	3·7	2 ,, 4
		41	$\frac{1}{2}$ to $1\frac{1}{2}$	$\frac{1}{8}$ to $1\frac{1}{4}$						
75	,,	22	$\frac{1}{8}$	0	75	,,	20	15	4·0	2 ,, 4
		43	$\frac{1}{2}$ to $1\frac{1}{2}$	$\frac{1}{8}$ to $1\frac{1}{4}$						
80	,,	23	$\frac{1}{8}$	0	80	,,	20	17	4·2	2 ,, 4
		45	$\frac{1}{2}$ to $1\frac{1}{2}$	$\frac{1}{8}$ to $1\frac{1}{4}$						
85	,,	24	$\frac{1}{8}$	0	85	,,	20	19	4·4	2 ,, 4
		48	$\frac{1}{2}$ to $1\frac{1}{2}$	$\frac{1}{8}$ to $1\frac{1}{4}$						
90	,,	26	$\frac{1}{8}$	0	90	,,	20	21	4·5	2 ,, 4
		50	$\frac{1}{2}$ to $1\frac{1}{2}$	$\frac{1}{8}$ to $1\frac{1}{4}$						
95	,,	28	$\frac{1}{8}$	0	95	,,	20	23	4·8	2 ,, 4
		51	$\frac{1}{2}$ to $1\frac{1}{2}$	$\frac{1}{8}$ to $1\frac{1}{4}$						
100	,,	30	$\frac{1}{8}$	0	100	,,	20	25	5·0	2 ,, 4
		52	$\frac{1}{2}$ to $1\frac{1}{2}$	$\frac{1}{8}$ to $1\frac{1}{4}$						

[1] The figures in this column are obtained by multiplying the weight of the ball in cwts. by the height of the fall in feet, and dividing by the weight of the rail per yard in lbs. The lighter the rails the lower the speed, and therefore the lighter the test in proportion.

This Paper is accompanied by several diagrams, from which the Figs. in the text have been prepared.

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APPENDIX.

The Weight and Section of Rails.

(From the "Iron Age," November, 1885.)

A committee of the Roadmasters' Association has submitted the following report upon the "Weight and Form of Rail, with Joint":—

1. *Weight.*—For the rail of the quality at present manufactured, and to suit the present spacing of cross-ties, your committee would recommend two standard weights—60 pounds per yard for light and 70 pounds per yard for heavy traffic.

2. *Form.*—They would recommend the base to be equal to the height, and the radius of the top of the head to be the same as that of the fillet of the wheel, ½ inch preferred.

3. *Joint.*—Your committee do not feel prepared as yet to recommend a joint. The double angle splice makes a superior joint, but inasmuch as this subject is at present receiving considerable attention in railroad circles, and as there are good forms of joint which merit attention, they would recommend that it be discussed and continued to the next meeting.

The section adopted is the same as that adopted by the Pennsylvania Road, in accordance with a recommendation of the Master Car-Builders' Association. The radius is ½ inch, but regarding this point the master car-builders have not come to any decision. The height of the rail is 4½ inches, and the width of web ½ inch. Radius of the upper part of the head is 12 inches. Some of the members thought that it was an open question what the best rail was, and disliked the idea of the Association going on record without having given both a practical trial. It was thought by others that the weight of the rail should be governed by the amount of traffic upon the different roads, and that a 75-pound rail would be about the thing for most of the roads in this country to use. The weight should not be considered as much as the quality of the rail, because, no matter how heavy it is, it is useless as soon as the surface becomes rough, and it must be removed. The only real reason for increasing its weight is in order that its stiffness may be added to, so as to enable it to carry the load required without danger of breaking. Its shape should be so as to conform as nearly as possible to the tread of the wheel. A heavier rail without increase of stiffness only added to the cost of the article without enhancing its durability.

The other half of this subject, namely, "rail joints," was next given attention. Discussion was started after a motion to the effect that the angle bar is the best splice was carried. The attempt to indorse the suspension of the angle splice was not so successful. It was believed by a good many that the suspended bar was what they would eventually come to, but that its present shape and their understanding of it was not just what its strongest advocates claim for it. It was true that more timber could be gotten under the track when the joint was suspended. William Dunn, of the Baltimore and Ohio, related his experience with a curve on his division. He could not get an angle bar long enough for his purpose, so he had an ordinary fish-plate made about 40 inches long, with which he made his joint. He put a good, stout tie under the joint, and one at each side at the proper distance. This was done last March, and a spirit-level now fails to detect any change in it. He believed in heavy, good, strong bars, and men who

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understood their business to take care of them. Others thought that a suspended joint looked quite pretty on a new track, but its results in service were not so attractive. If a good one was attained, it certainly was a benefit in some respects, especially in the saving of material.

M. J. McInarna, New York, Lake Erie and Western, said that the best rail joint is that which supports the ends of the rails on the same principle as the one invented and manufactured by Mr. Clark Fisher. It forms a bridge across two ties laid six inches apart in the clear, and the rails are fastened to it by means of a U-bolt that holds them in place, and prevents them from working up and down under every passing wheel. This working up and down is injurious to the ends of the rails. By the constant wear they become bruised, which causes pounding of the wheels, and renders it impossible to keep the joint up in proper surface. This of course causes the removal of the rails while they are yet practically good. This he thought was the result wherever the suspension joint with the angle splice is used. He believed in putting a tie under the joint, whether fish-plates or angle-bars were used. Some thought that a suspension joint was serviceable when the plates did not exceed 24 inches in length.

The matter was finally disposed of by the Association going on record by passing the resolution, "That it is the sense of this meeting that the length of the angle joint should be 44 inches long, or to reach three ties, with a tie under the centre."

European Railway Progress.

(*From "Engineering," December*, 1885.)

Some statistics of considerable interest have just been collected in illustration of the progress of railway construction in Europe in 1884. It appears that at the close of 1884 there were 118,334½ miles of railways in operation upon the European Continent, as compared with 114,374½ miles at the close of 1883. It follows that 3,959⅝ miles of new line were opened in Europe last year. The increase, it will be seen, is equivalent to nearly 3½ per cent. upon the 114,374½ miles of line at work in December, 1883. The 3,959⅝ miles representing the new line brought into operation in 1844 was made up as follows: Germany, 518½ miles; Austria and Hungary, 780⅛ miles; Belgium, 28⅜ miles; Denmark, 81⅞ miles; Spain, 257½ miles; France, 938⅜ miles; Great Britain and Ireland, 209⅜ miles; Greece, 95⅛ miles; Italy, 293⅜ miles; the Low Countries and the Luxemburg, 83⅛ miles; Portugal, 20⅛ miles; Roumania, 51⅞ miles; Russia and Finland, 326⅛ miles; Servia, 152½ miles; Sweden and Norway, 126½ miles; and Switzerland, 6⅞ miles. The extent of line in operation in Germany at the close of 1884, was 22,960⅛ miles. France ranked second with 19,510 miles; Great Britain and Ireland, third, with 19,071½ miles; Russia and Finland, fourth, with 15,869⅜ miles; and Austria and Hungary, fifth, with 13,816½ miles.

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(Paper No. 2050, Supplement.)

SINCE the Paper "On Rail-Joints and Steel Rails" was written it has been suggested that it would be well to specify a more definite remedy for the present state of roads. It is in response to this suggestion that the Author now supplements his work by recommending particular alterations in the rail in question. In the introduction to the Paper the Author stated that his object was to warn European railway companies to avoid errors committed elsewhere. At the same time if, by stating how rail-joints and steel rails were employed in Europe, Americans could be led to some explanation of the phenomena which they observed, so much the better; but the Author had no pretension to solve the American railroad problem; for he was only acquainted with their workings from inspections of rails for some of the leading American railways.

WEAK RAIL-JOINTS AND LIGHT RAILS.

Why do rail-joints break? Because they are not strong enough, was the answer of a correspondent of "The Railroad Gazette." And why do rails wear out so soon? Because they are too light. These simple explanations were as near the truth as they could be, and had only the fault of being too simple. Science was therefore called upon to make other suggestions, but, notwithstanding, the result will be the same—namely, that rail-joints break largely, and rails wear badly.[1] In Europe affairs have not arrived at such a critical state, but they are in a fair way of approaching it, and this is all the more to be regretted, inasmuch as there is no reason for saving, there being little or no duty upon American rails. Admitting that in Europe as well as in America the flange-rail is the cheapest and best for a new country, it must, on the other hand, be granted that since the traffic has developed on certain main lines for international through traffic so as to be tripled, and the rolling-stock has accordingly been augmented in weight and the speed increased, the permanent way should be strengthened in proportion. This is just what is needed, and a calculation will

[1] "The Railroad Gazette," 1886, vol. xviii. p. 194.

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now be made whether it would not be economical to adopt a stronger permanent way for these portions, removing the lighter rails for service on the branches with lighter traffic. It was stated by Mr. B. Baker, M. Inst. C.E., in his address last year before the British Association, that many repeated charges of loads on a structure must try the material very much more than a constant stress, and even very light stresses, if repeated many million times, will cause breakage. Hence the results of single tests on rails are not at all comparable with the cases in which millions of trains pass over the rails. Suppose, for instance, that a rail can carry 30 tons in one test, it may yet break with a weight of only 5 tons after millions of repeated charges. The fact should further be taken into account that the sleepers are by no means fixed supports, but press down in the ballast if the rail is weak, so as to spring under the load. With reference to this remark it is pointed out in "The Railroad Gazette" that want of weight of rail in America is counterbalanced by the sleepers being generally laid 2 feet apart instead of 3 feet, as in Europe: it must be admitted that this is to a certain extent of some value in favour of the American light rails; but after all it carries very little weight in practice, inasmuch as the light rail will bend and dislocate the sleeper and the ballast. Another method, by which American railway engineers have attempted to lessen the effect of disproportionately heavy rolling-stock upon light rails, is to divide the weight of the engine among many axles and wheels, say twelve wheels, keeping to a low weight for each wheel, such as 5 tons, but still coming up to a total of 60 tons for the engine, whence it is argued that the weight for each wheel is only 5 tons, which ought not to be too much upon a 60-lb. rail. But this is only applicable to dead loads on fixed supports, or where the engine stands; for when running at a high speed the rail has no time to recover its original position for each wheel, the sleepers being sunk down into the ballast, and consequently the total weight of the engine must be taken as a charge upon the rails.

Opposite practice in the working of Railways.

In consequence of the growth of traffic, engines have been increased in weight from 30 to 60 tons, and carriages from 10 to 20 tons, while the rails have been kept to their original weight, 60 lbs. or 70 lbs. per yard. What could the result be under such circumstances but fatal both to the rail-joint and to the wearing

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of the rail? Calculating that which should really be employed, comparing the flange-rail section used in America and on the Continent of Europe with the bull-headed rail used with chairs on English railways, it will be assumed that English engines weigh on the average 40 tons, the carriages being very light. For this rolling-stock the rails weigh 80 lbs. to 90 lbs. per yard, with a cast-iron chair of 40 lbs. per yard, which, taken at half the value of the steel, would increase the weight of the flange-rail by 20 lbs. per yard, that is, from 100 to 110 lbs. per yard. But, if a 40-ton engine requires 110-lb. rails, a 60-ton engine should require half as much again, or one of 150 to 165 lbs., in order to be equal in strength and capacity to that of the English roads. For there is no reason why steel in the form of a flange-rail should do more work than one in the form of a bull-headed rail. The bull-headed rail could be rolled much harder than the flange-rail without risk of breakage, and therefore a smaller weight in the former ought to correspond with the heavier flange-rail; yet it is just the contrary in practice. Taking the average weight of rails at 60 lbs. to 70 lbs. per yard, as used in America and on the continent of Europe, it does not come up to half that which it ought to be according to English practice. Now, is the English practice right or wrong? English railways represent an enormous capital, some of them more than £100,000,000 sterling, and most of the large lines pay a 6 to 7 per cent. dividend yearly. In addition to the dividend they afford greater safety and comfort than any line laid with flange-rails. How is it that such a vast difference in the fundamental engineering bases for working railways can have crept in? In England light engines are run on heavy rails, and in America and on the Continent of Europe heavy engines on light rails. If the comparative cost of renewal of rolling-stock and of permanent way in countries where the opposite practices prevail be examined, the difference will be found to be very great, for while English engines and carriages, through the perfection of the roads, need very small repairs in the year (see Table on opposite page), American and Continental rolling-stock will probably have to be in the repairing shops frequently and for long intervals, to say nothing of the cost of repairing the road. Look again at the steady increase of the weight of the rail in England. No Government regulations have forced English engineers to augment the weight. However, they have done so for the last twenty years, increasing it from 70 lbs. to 90 lbs. per yard as a matter of economy. Again, that which was economy with the price of rails fifteen years ago at

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AVERAGE MILEAGE of LOCOMOTIVE ENGINES BETWEEN SHOP REPAIRS, GREAT WESTERN RAILWAY.

Class.	Mileage.		
	Highest.	Lowest.	Average.
Passenger engines—			
7-feet express engines, single driving wheels	71,400	24,000	52,000
6½-feet coupled express engines .	79,600	21,000	54,200
5-feet coupled tank engines . .	94,000	26,000	48,000
Goods engines—			
Six-wheels coupled tender engines, 5-feet wheels	68,300	17,300	42,200
Six-wheels coupled heavy saddle tank-engines, 4½-feet wheels .	55,700	17,000	33,500

£16 per ton, can by no means be said to be economy at the present time with their price at only £4 per ton. The saving of labour for renewals and maintenance is now the vital question in the cost of railway working, and when this is brought to a minimum by perfection of road, a dividend is obtained at the same time that safety and comfort are secured in travelling. In the present state of things the rail-maker is put between two fires. He is limited to a very thin flange-section of light weight, to chemical composition and mechanical tests, and even a guarantee; if he makes the rails hard they break under the heavy rolling-stock, while if he makes them soft they will wear out in a few years. What then is he to do? To save his reputation he will naturally keep on the safe side, but the result can by no means be economical to the railway. On the Continent of Europe there are many roads with heavy engines, yet the rail is limited to its original weight, notwithstanding the increase of traffic. Rail-sections are much deeper than in America, but there is only a very small head and a very narrow flange upon the rail, and a thin high web. In America, the duty which increases the cost of the steel rail 50 per cent. is certainly a great inducement to save metal, but that does not make the rail any stronger. Of course quality will go a long way, such as using a pure strong material, a mixture of Swedish iron, hammering of the blooms, perfection in execution of rolling, and the best inspection. All this will be well worth considering if a minimum of weight must be maintained. The Author, having been brought up in the midst of Swedish ironworks, is certainly the first to appreciate good material, but there is a limit beyond which even this can be overloaded, and then the remedy is more metal.

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On the Continent of Europe many of the railways are in the hands of the Government, with heavy administrative and other difficulties in the way of moving out of a groove once entered into. But wonders cannot be accomplished with flange-rails, and the only natural way is to allow sufficient metal in the form of rail, with the flange-section, as well as with the bull-headed rail, always in proportion to the work which has to be done.

Metal Sleepers.

The enormous consumption of wooden sleepers, and their rapid decay, particularly in hot countries, makes it an absolute necessity to substitute metal for wood; and therefore it is best to face the conditions at once and make metal sleepers substantial. Besides, wood is getting dearer day by day and metal cheaper.

The substitution of metal sleepers for wooden ones is of course entirely a question of locality and of price. In hot countries the metal sleeper is especially advantageous.

The phenomenon of steel rusting faster than iron, particularly when exposed to moisture, is an element of great importance in the metal-sleeper question, inasmuch as steel, being more easily rolled, is more and more largely substituted for iron in the form of sleepers. The Author suggested one explanation of this phenomenon in his Paper, but it seems however to be better explained in the "Engineering and Mining Journal," of New York, for February 6th, 1886. It is there stated that the rapid rusting is due to the alloy of iron and manganese, the latter being a highly oxidizable metal. In this view the Author agrees; and as steel is combined with very different proportions of manganese, varying from 0·25 per cent. up to 1 per cent., it would be of interest to compare the rapidity with which oxidation takes place in metal of such extremely different composition. Not only would this comparison be of interest for the question of rails and steel sleepers, but also for boiler-plates, ship-plates, and particularly for bridge work. If the suggested explanation should be confirmed by experiment, then the use of such steel as contains the least proportion of manganese would of course be preferable.

If equal safety is to be obtained, the metal sleeper must be of equal size and cover an equal surface of the ballast as the wooden sleeper; and before all else the rail must be stiff enough to distribute the load, or the effect of the concussion of the rolling stock, among many neighbouring sleepers, instead of concentrating it upon one or two as in the case of light rails. A big-headed rail

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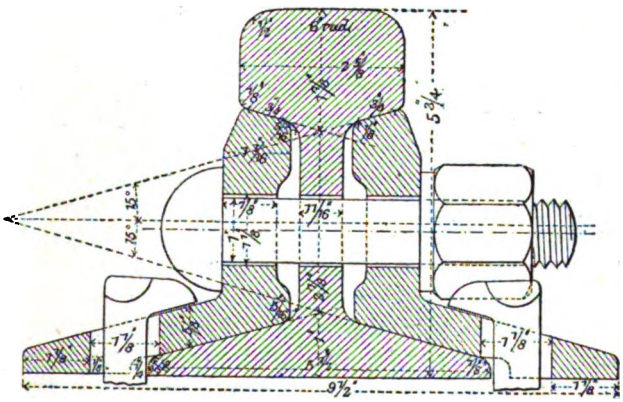
is also required, for the same reason that a blacksmith uses a heavy anvil for a heavy blow of the hammer. The great number of forms of metal sleepers, and the modes of fixing the rail to the same, must be puzzling to an engineer who has to select one of them. The simpler the plan the better. But any system will prove a failure within a very short period if used with a rail that is light compared with the rolling-stock. Indeed experience on the Continent of Europe has proved this during the very short period they have been laid down; not only are the joints too weak, but the whole of the system is shaky, and will by no means do justice to the name of "permanent" way.

A 100-LB. RAIL NEEDED.

With this mode of obtaining a perfect permanent way with the heavier form of flange-rails for roads with heavy traffic, say a rail in proportion to the rolling-stock according to English practice; a sleeper, whether of wood or iron, that will not dislocate the ballast; and the joint as strong as the rail, so as to give one line of



SANDBERG'S 100-LB. STANDARD RAIL.

Scale ½ full size.

Rail to carry 50-ton engine running at a speed of 50 miles per hour. Depth of rail, 5¾ inches. Proportion of rail-section: head, 43 per cent.; web, 22 per cent.; flange, 35 per cent. Rail equal in cost and in capacity to the 80-lb. English rail, including cast-iron chairs.

continuous stiffness, the Author suggests the adoption of a 100-lb. flange-rail as shown in the annexed Fig. The rails should be joined according to local circumstanees, climate, &c. Where there is snow and ice, they should be joined with angular fish-plates; and in countries with a mild climate with deep fish-plates, giving

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an equal stiffness to the rail itself. Fortunately for the adoption of heavier rails the extra cost at present will be reduced to next to nothing, in consequence of the fall of the price of rails, at least in Europe. But even taking it per weight as increasing 50 per cent. of the metal in the rail per mile from 66 lbs. per yard up to 100 lbs., or 50 tons extra per mile, that would at the present price of, say £4 a ton, only come to £200 a mile. This rail has got twice as much wearing surface as the 66-lb. rail, and consequently ought to last twice as long. Taking also into account the saving in maintenance of rolling-stock and road, and the recovery of 50 per cent. of its value when worn out, it would certainly be but a very small matter for many railway companies to make a trial with this rail for the most heavily worked parts of their line, and it is probable that after a few years' experience they would not go back to the light rails. The 100-lb. flange rail-section would be, if anything, cheaper than the ordinary English line, with 80-lb. bull-heads and chairs. The section shown in the drawing is designed on the same basis as the Author's standard rail-sections, of which such large quantities have been made, and have given satisfaction alike to producers and consumers.

Strengthening of Rail-joints.

But what is to be done with the already existing weak rails and joints laid down upon the present systems? They cannot be taken up all at once for the substitution of heavy rails. The weakest points upon the existing roads laid with flange-rails are the joints depending upon the angle for fishing. If this is as large as it is in many cases on the Continent of Europe, say 60°, instead of 30° as it should be, the bolts will constantly work loose, and the joint-sleepers sink in the ballast. In such cases there is no possibility of strengthening the joint by applying new strong fish-plates. The best way, in the Author's opinion, is to apply a bridge-plate covering the two joint-sleepers, either of the Fisher or the Elworth plan, which can be adopted for wooden or for metal sleepers. But where the section of rail is good for fishing, say with an angle of 30°, stronger fish-plates can be used either upon the American or the Swedish pattern described in the Paper. Anyhow, the strengthening of the joint is but a temporary palliative as compared with laying down a heavier rail with a perfect joint of sufficient strength to correspond to the traffic and weight of the rolling-stock.

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Conclusions.

Thus, in conclusion, the practical suggestion for the improvement of the present state of roads laid with flange-rails which have become overloaded, is to employ as heavy a rail in that form as would compare with the English practice with rails of bull-headed section. The Author would advise every engineer interested in the comparison to travel upon the English roads from London to Scotland and back, as well as to ascertain the cost of maintenance and renewals of the same. As for safety and comfort the journey alone would be sufficient to convince him, and he would also conclude that the heavier flange-rail would ensure the success of metal sleepers, while the patching up of the joints by stronger fishes or by bridge-plates would only be a temporary expedient until financial circumstances should allow the more radical remedy of giving sufficient metal to do the heavier work.

In suggesting a 100-lb. rail, the Author has met with less opposition than was at first expected, for to propose a sudden increase of 50 per cent. in the weight of rail is certainly a startling move. However, there are many railway engineers, both State and private, declaring themselves willing to make the experiment with the 100-lb. flange-rail. It must be clearly understood that the Author in making these suggestions is principally actuated by motives of pure interest in the work of railway engineers, with the object of improving roads laid with flange-rails, which by increased traffic have become too light, so that they come up to the same standard as those laid with the bull-headed section. If he had suggested as a remedy the adoption of bull-headed sections, or simply a copy of the English type of roads, very few American and Continental engineers would have listened to him. There is no fault or discrepancy in the pattern of the flange-rail, but, on the other hand, no one could expect to succeed with rails of that form having one-half the metal only. Yet this is what it has come to by keeping in the old groove adopted in years gone by, and not changing with the progress of the times in the increase of traffic to triple, and the lowering of the price of rails to one-third, which tells in both directions. On the other hand, if the rail makers in the present depressed state of trade would gain a temporary increase of output, that should certainly be no objection to the adoption of the heavier system, but at the same time this is not the motive of the Author's suggestions. The maker's gain would after all be only temporary, for these rails would last twice as long as the others, not only

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because of their greater substance, but because their form enables the engineer to demand a harder material without risk of breakage, and this harder material would wear longer than the softer. It would therefore be to the makers only a temporary gain in increase of production. As it was said by some engineers that the first Paper was quite true in its criticisms of existing deficiencies, but that it simply recommended harder and heavier rails, without any definite suggestion how to act, the Author has now tried to comply with the wishes of his critics by giving his suggestions in greater detail, with a drawing of a 100-lb. standard rail-section;[1] by recommending the substitution of metal for wooden sleepers, and the strengthening of rail-joints on existing roads; and by specifying such tests as he should use.

The Paper is accompanied by a diagram, from which the Fig. in the text has been prepared.

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[1] See the Table at the end of the Paper (No. 2137), in which the so-called barbarous tests have been still further reduced.

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(*Paper No. 2149.*)

"On the Rate of Hardening of Cement and Cement Mortars."

By PROFESSOR WILLIAM CAWTHORNE UNWIN, B.Sc., M. INST. C.E.

A VERY large mass of experimental data has now accumulated as to the strength of cements, and especially of Portland cement at various ages after mixing. The mechanical tests carried out in this country by Mr. Grant and others, and abroad in the State laboratories at Berlin and Munich, have thrown a great deal of light on the behaviour of cements, and have led to a great improvement of the manufacture.

So far as tests of cements are intended merely as a check on the manufacturer, they are necessarily carried out in a limited time. Seven-day tests, or at most twenty-eight day tests, are alone possible. But obviously such tests are, to some extent, imperfect for the purpose in view. The engineering value of a cement does not depend on the strength soon after it is mixed, but on the strength which accrues to it after a longer interval of time. Now the results of seven-, or even of seven- and twenty-eight-day tests, leave much uncertainty as to the probable strength of the cement after say six months, or a year. One cement has greater initial strength, another gains strength at a greater rate with lapse of time. To infer the probable engineering value of a cement, both the initial strength and the rate of gaining strength must be considered, and to infer the probable ultimate strength something must be known of the law of increase of strength.

Further than this, even when experiments are made at longer intervals of time, a difficulty arises. Notwithstanding the greatest care in preparation, the briquettes have a considerable variation of strength. Hence a series of experimental results shows discrepancies, and if such results are plotted with the times as abscissas and the strength of the briquettes as ordinates, the result is instead of a fair curve, a broken line, from which it is difficult to draw conclusions. A knowledge of the law of hardening, even of an approximate kind, would enable a fair curve to be drawn through such discrepant observations, and would greatly clear up the meaning of the results.

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A rational formula for the strength of cement of different ages is not to be hoped for; but it appeared to the Author, that experiments might furnish a law of an empirical kind simple enough to be easily used, and yet agreeing well enough with a sufficiently wide range of experiments to be relied on within reasonable limits.

Let y be the strength of a cement, or cement mortar, in lbs. per square inch, at an age of x weeks after mixing. From some preliminary tentatives the Author found for the relation between x and y, the expression

$$y = a + bx^n \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where a, b, and n, are empirical constants. In this form the equation would not be very convenient to use, for it would require at least three sets of experiments at three different ages of the test-pieces to determine the three constants; and a still greater number would be required for satisfactory results in consequence of the discrepancies which occur in cement-testing.

It appeared, however, that for any given kind of cement, and any given kind of straining action, n had a constant value. Further, by a modification of the formula, a might always be the strength of the cement at seven days' age. Consequently there would remain only one constant to determine. The formula then is

$$y = a + b\,(x - 1)^n \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where y is the strength of a cement or mortar, at x weeks after mixing, the initial strength of which at seven days is a lbs. per square inch. The constant n has values which can be assigned beforehand, and only the constant b remains to be determined by experiments on test-pieces more than one week old. It will be seen, hereafter, that though b varies, its variation is not within very wide limits, so that when the characters of cements are better known it may be possible to assign for it a probable value, even if experiments are wanting in any given case.

Now since in this equation a is the initial strength of the cement, and b a constant, varying with the rate of increase of strength with time, the two constants exhibit very clearly the character of a cement. If their values are determined for any given cement, and inserted in the equation, a numerical equation is obtained which may be termed the characteristic equation for the cement.

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Tension-Tests of Portland Cement.

To examine the applicability of the formula, let the constants be determined for the series of tests of Portland cement briquettes extending over seven years given in Mr. Grant's first Paper.[1] Mr. Grant's Table gives in each case the mean strength of ten briquettes, $2\frac{1}{4}$ square inches in section. In one series the cement was gauged neat; in another series the cement was mixed with an equal weight of clean Thames sand. Mr. Grant's numbers reduced to lbs. per square inch are as follow :—

Age.	Strength per Square Inch.	
	Neat Cement.	1 Cement + 1 Sand.
7 days	363	157
1 month	415	202
3 months	470	244
6 „	525	285
9 „	542	307
12 „	547	320
2 years	590	351
3 „	585	350

Now for Portland cement in tension $n = \frac{1}{3}$; the constant a has the values 363 and 157 for the two series, and it only remains to determine the most probable value of b in the equation—

$$y = a + b\sqrt[3]{x-1}$$

This is best done by calculating b for each of the experiments, except that at seven days, and taking the mean of the values so found. Thus—

Neat Cement.

Age. x.	Strength. y.	$y-a$.	$b = \frac{y-a}{\sqrt[3]{x-1}}$.	Strength. y by Formula.
1	363	..	..	363
4	415	52	36	431
13	470	107	47	471
26	525	162	56	503
39	542	179	53	525
52	547	184	49	541
104	590	227	48	588
156	585	..	..	..
	Mean		48	

[1] Minutes of Proceedings Inst. C.E. vol. xxv. p. 89; also vol. xxxii. p. 280.

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1 CEMENT + 1 SAND.

Age. x.	Strength. y.	$y - a$.	$b = \frac{y-a}{\sqrt[3]{x-1}}$.	Strength. y by Formula.
1	157	..	..	157
4	202	45	31	214
13	244	87	38	249
26	285	128	44	274
39	307	150	45	292
52	320	163	44	305
104	351	194	41	345
156	350	193	36	372
208	363		—	
260	365	Mean . . .	40	

Hence the characteristic equations for this cement and cement mortar are—

Neat cement . . . . . $y = 363 + 48\sqrt[3]{x-}$

Cement mortar . . . . . $y = 157 + 40\sqrt[3]{x-1}$

Hence the sand reduces the initial strength of the cement by rather more than one-half (from 363 to 157 lbs. per square inch), and the gain of strength at any age is less for the mortar than for the neat cement in the proportion of 40 to 48. By comparing the calculated and observed values of y, it will be seen that the formula agrees closely enough with experiment for practical purposes. Mr. Grant's figures show that the neat cement reached its maximum strength in one hundred and four weeks, and the cement and sand in one hundred and fifty-six weeks. Hence values of b are calculated from data up to those dates only.

In Fig. 1 the experimental values are shown by small circles, connected by a broken dotted line, and the calculated values lie on the curves.

In the case of cement mortar the briquettes gain in strength up to any period to which experiments are usually extended. But with neat cement briquettes, a maximum strength appears to be reached, often in about three months, after which the strength remains constant or slightly falls off. For example, in the experiments given by Mr. Grant on the relative strength of briquettes made on an impervious slab, and on a porous (gypsum) slab,[1] the strength slightly diminishes after thirteen weeks. Hence

[1] Minutes of Proceedings Inst. C.E. vol. lxii. p. 142.

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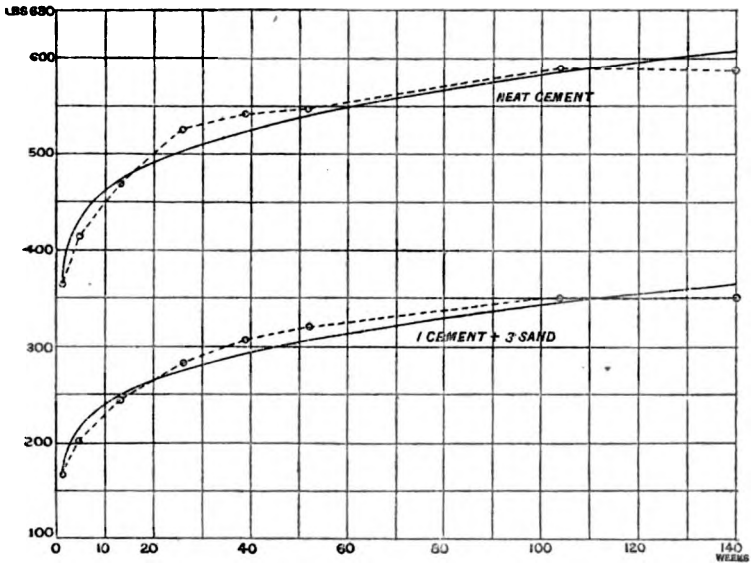
for such results the value of b must be deduced from experiments before the maximum is reached, and the formula ceases to apply

FIG. 1.



beyond the maximum. The following characteristic equations were deduced for these experiments from the results for four, eight-and-a-half, and thirteen weeks.

NEAT CEMENT.

Impervious slab . . . . . $y = 449 + 161\sqrt[3]{x-1}$;
Gypsum slab . . . . . $y = 257 + 164\sqrt[3]{x-1}$.

x Weeks.	Impervious Base.		Gypsum Slab.	
	y Observed.	y Calculated.	y Observed.	y Calculated.
1·0	449	449	257	257
4·0	687	681	454	493
8·5	765	765	634	577
13·0	808	819	626	631
26·0	731	..	620	..
39·0	746	..	655	..
52·0	718	..	608	..

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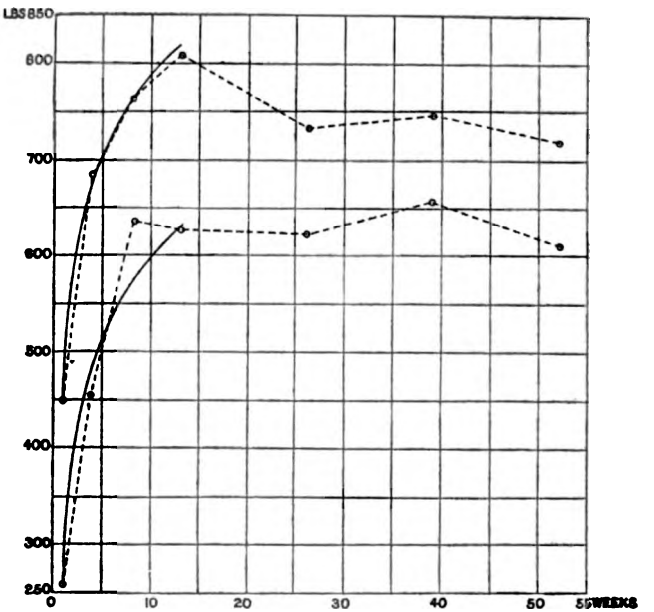
These results are plotted in Fig. 2. The gypsum-slab results are somewhat irregular, but the theoretical curve strikes a fair balance between the observations.

FIG. 2.



In Table XXX. p. 149, of Mr. Grant's Paper, is given a very complete series of experiments on the effect of fineness of grinding on the strength of briquettes of neat cement, and cement and sand. Part of the briquettes were made with coarsely-ground cement, having 10 per cent. residue on a sieve of 50 meshes, and part of the same cement sifted through a 180-mesh sieve. The experimental results furnish the following equations:

Neat cement, unsifted.

$$y = 353 + 122\sqrt[3]{x - 1}.$$

The same, sifted.

$$y = 346 + 36\sqrt[3]{x - 1}$$

Unsifted cement and sand, 1 to 3.

$$y = 75 + 69\sqrt[3]{x - 1}.$$

Sifted cement and sand, 1 to 3.

$$y = 252 + 53\sqrt[3]{x - 1}.$$

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Unsifted cement and sand, 1 to 5.

$$y = 31 + 46\sqrt[3]{x - 1}.$$

Sifted cement and sand, 1 to 5.

$$y = 136 + 47\sqrt[3]{x - 1}.$$

Figs. 3, 4, 5 show these results plotted as before.

Here the reduction of strength by the addition of sand, and the superiority of the sifted cement when mixed with sand are more easily seen in the equations than in Mr. Grant's Table. Further, the exact nature of the effect of sifting the cement can be gathered.

FIG. 3.

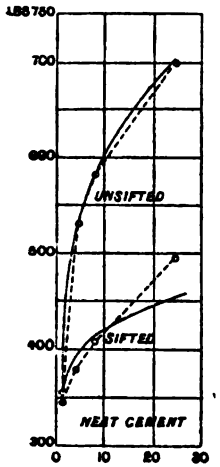


FIG. 4.

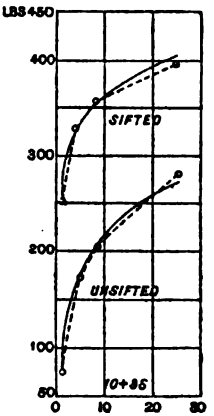
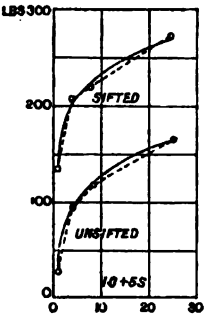


FIG. 5.



With neat cement increased fineness diminishes the gain of strength with age, that is, the constant b diminishes. When mixed with sand, the gain of strength with age is the same whether the cement is sifted or not, but with the sifted cement the initial strength is greater.

The following Table gives the observed strength in lbs. per

Age of Briquette in Weeks. x.	Unsifted Neat.		Sifted Neat.		Unsifted 1 C. + 3 S.		Sifted 1 C. + 3 S.		Unsifted 1 C. + 5 S.		Sifted 1 C. + 5 S.	
	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.
1	353	353	346	346	75	75	252	252	31	31	136	136
4	533	529	380	398	171	174	330	328	97	97	208	204
8	585	586	409	415	206	208	358	353	118	119	223	225
25	701	705	495	454	282	273	397	405	166	164	272	271

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square inch, and the strength calculated by the equations given above. The agreement is obviously very satisfactorily close.

Again, Mr. Grant gives at p. 143, experiments on the influence of different finenesses of sand on the strength of the cement mortar. In this case, the strength at one week is not stated; but by inversion of the formula it is easily obtained. The characteristic equations are found to be—

1. Sand passing a 20-mesh and retained on a 30-mesh sieve. Pressed—

$$y = 73 + 31\sqrt[3]{x - 1}.$$

2. The same sand. Ordinary pressure—

$$y = 89 + 19\sqrt[3]{x - 1}.$$

3. Coarser sand, passing a 10-mesh, and retained on a 20-mesh sieve—

$$y = 172 + 28\sqrt[3]{x - 1}.$$

The following Table gives the observed and calculated strength:—

Age. Weeks.	(1.)		(2.)		(3.)	
	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.
1·0	..	73	..	89	..	172
4·0	117	117	116	116	212	212
8·5	134	134	126	121	237	227
13·0	145	144	124	132	206	236
26·0	156	163	135	144	253	253
39·0	158	177	160	153	268	266
52·0	213	190	182	160	274	279

Here again the discrepancies do not appear greater than the probable errors of the experiments themselves. The differences between successive observed values are not regular as they should be, and in the strength of (3) at thirteen weeks, the observed value must be nearly 40 lbs. too low.

The influence of different proportions of sand on the strength of cement mortar, is shown clearly in the following values of the constants deduced from a very regular and complete series of experiments in the report on the Main Drainage Works at Boston, by Mr. E. C. Clarke. The experiments were extended to a year, and fit the formulas very closely.

Neat cement	$y = 303 + 61\sqrt[3]{x - 1}$
1 cement + 1 sand	160 + 57
1 cement + 2 sand	126 + 44
1 cement + 3 sand	95 + 36
1 cement + 5 sand	55 + 26

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In the same report there are some good experiments on the effect of fineness of grinding of the cement. A cement was sifted through a 120-mesh sieve, and the coarse and fine portions were mixed in different proportions. Briquettes made with three and five parts of sand gave the following results:—

Percentage of Coarse Particles in Cement.	1 Cement + 3 Sand.	1 Cement + 5 Sand.
55	$y = 39 + 28\sqrt[3]{x-1}$	$y = 19 + 19\sqrt[3]{x-1}$
33	92 + 42	48 + 32
28	97 + 45	47 + 35
18	117 + 44	65 + 35
8	123 + 50	73 + 36
0	154 + 44	86 + 35

The Author has examined a great number of experiments in this way, and in no case are the discrepancies between the calculated and observed values materially greater than in these Tables. The formula may, therefore, be taken as a sufficiently accurate formula of interpolation. It will be useful next to examine what is the range of value of the constants for different qualities of cements.

Values of Constants for different Cements.

First some data will be selected from the large Table XLVII., p. 169, of Mr. Grant's Paper. The only principle of selection adopted was to take those cements for which the completest series of data are given. The following Table gives the characteristic equations obtained for neat cement briquettes, the constants being in all cases deduced from the strengths given at one, four, and thirteen weeks. In none of these cements did the strength increase beyond that period.

Characteristic Equations for Mr. Grant's Cements.

Units, lbs. per square inch, and weeks. Gauged neat.

No.	Water used per cent.	Setting Time in Minutes.	Characteristic Equation.	Initial Strength.	Strength at 13 weeks.		Strength at 52 weeks.
					Observ.	Calc.	
1	18·5	Slow	$y = 558 + 96\sqrt[3]{x-1}$	558	815	778	825
12	22·5	15	„ 422 + 118 „	422	744	692	694
13	20·0	600	„ 614 + 91 „	614	846	824	813
„	21·2	„	„ 610 + 91 „	610	840	820	779
„	22·5	„	„ 520 + 112 „	520	783	780	772
15	20·0	480	„ 626 + 56 „	626	824	755	718
„	22·5	„	„ 576 + 140 „	576	833	896	785
19	22·5	„	„ 422 + 188 „	422	793	852	747

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The tests of cement which form the most complete series are, however, to be found in the publications of the Engineering Laboratories of Munich and Berlin. In the Mitt. aus den mech. techn. Laboratorium in München, for 1879, there is a remarkable series of experiments on the tensile strength of cements, by Professor Bauschinger. In Table II. are given no less than three hundred and sixty results, each the mean of ten separate experiments. Ten different cements were used, and these were made into test-pieces of 72 square centimetres (11 square inches) section. From these results the Author obtained the following equations, those for neat cements being deduced from results on test-pieces one week to sixteen weeks old, and those for cement and sand from results on test-pieces one week to one hundred and nine weeks old.

The extreme regularity of the constants for a great variety of cements and their limited variation in value is remarkable. The comparatively low initial strength may be partly due to the cement being fresh, partly to the size of the test-pieces.

CHARACTERISTIC EQUATIONS for TENSILE STRENGTH of BAUSCHINGER'S CEMENTS.

Units, lbs. per square inch, and weeks.

Cement Mark.	Setting Time. Minutes.	Neat Cement. $y =$	1 Cement + 3 Sand. $y =$	1 Cement + 5 Sand. $y =$
A	80	$199 + 40\sqrt[3]{x-1}$	$81 + 29\sqrt[3]{x-1}$	$43 + 31\sqrt[3]{x-1}$
B	49	$121 + 49$	$46 + 36$	$26 + 30$
C	195	$199 + 95$	$108 + 40$	$78 + 37$
D	136	$199 + 53$	$53 + 61$	$37 + 47$
E	13	$185 + 42$	$85 + 40$	$60 + 28$
F	416	$256 + 37$	$112 + 41$	$67 + 38$
G	9 to 17	$227 + 64$	$91 + 40$	$67 + 33$
H	14 „ 26	$227 + 36$	$80 + 46$	$50 + 36$
R	344	$299 + 61$	$136 + 37$	$105 + 31$
T	146	$227 + 61$	$114 + 28$	$71 + 24$
		Mean 54	Mean 40	Mean 33·5

Parts of the same test-pieces were subjected to shearing. The following are equations for a few of them:—

Cement Mark.	Neat Cement. $y =$	1 Cement + 3 Sand. $y =$	1 Cement + 5 Sand. $y =$
A	$270 + 43\sqrt[3]{x-1}$	$112 + 49\sqrt[3]{x-1}$	$60 + 52\sqrt[3]{x-1}$
B	$142 + 67$	$46 + 53$	$31 + 57$
R	$412 + 57$	$185 + 55$	$131 + 61$
T	$242 + 92$	$131 + 43$	$105 + 44$

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Here the constants are somewhat higher than for tension; but in other respects are similar.

In the Mitth. aus den kon. techn. Versuchsanstalten zu Berlin for 1885, there is a very extensive series of tests of cement by Dr. Böhme, the test-pieces being the ordinary German standard test-pieces of 5 square centimetres section. The object of the research was to determine the influence of some additions to, or adulterations of, cement on its strength. Hence these results will be useful in showing whether the characteristic equation exhibits clearly the peculiarities of the cement.

The cement mixtures used were:—

A. Pure cement of normal fineness.

B. 9 per cent. of the same cement and 10 per cent. of the finest part of the cement sifted through a 180-mesh sieve.

C. 50 per cent. of cement and 50 per cent. of finest sifted cement.

D. 90 per cent. of cement and 10 per cent. of brickdust.

E. 50 per cent. of cement and 50 per cent. of brickdust.

F. 90 per cent. of cement and 10 per cent. of lime.

G. 50 per cent. of cement and 50 per cent. of lime.

And other mixtures the data for which the Author has not reduced. Each cement mixture was tried gauged neat, and also with 1 to 3 of normal sand. The equations obtained were these:—

Cement Mixture.	Gauged Neat. $y =$	1 Cement mixture + 3 Sand. $y =$
A. 100 cement	$586 + 99\sqrt[3]{x-1}$	$199 + 47\sqrt[3]{x-1}$
B. 90 „ + 10 fine cement	$499 + 99$	$214 + 44$
C. 50 „ + 50 „	$432 + 112$	$228 + 51$
D. 90 „ + 10 brickdust .	$452 + 107$	$137 + 58$
E. 50 „ + 50 „	$244 + 114$	$74 + 53$
F. 90 „ + 10 lime . .	$514 + 71$	$155 + 48$
G. 50 „ + 50 „ . .	$262 + 88$	$82 + 44$

For the briquettes gauged neat, the maximum strength is reached in thirteen weeks or a little more, and the equations do not apply beyond that time. The same is true of the briquettes made with sand in the case of the addition of lime (F and G). The remaining briquettes made with sand gained regularly in strength up to fifty-two weeks. It will be seen that the addition of fine cement reduces the strength in neat cement briquettes, and increases the initial strength without much altering the rate of gain of strength in briquettes made with sand. The brick-

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dust diminishes the initial strength, but increases the rate of gain of strength. The lime diminishes the initial strength, and diminishes the gain of strength.

Compression-Tests of Portland Cement.

It seemed worth while to try if a similar equation would apply equally well in the case of compression-tests. In the Paper by Bauschinger already referred to, there is a series of compression-tests quite as extensive and complete as the series of tension-tests.

The test-pieces were cubes of 144 square centimetres (22·3 square inches or 4·75 inches length of side). Either from the large size of the cubes, or the nature of the stress, or some other cause, it is necessary to take $n = \frac{1}{2}$ in the general equation, instead of one-third its value for tension. With this change, the equation fits the compression results satisfactorily. The equation for compression is therefore—

$$y = c + d\sqrt{x - 1}.$$

where y is the compressive strength in lbs. per square inch at x weeks after mixing.

The equations obtained from Bauschinger's results, are as follow, the equations being all applicable to an age of two years at least.

Cement Mark.	Neat Cement. $y =$	1 Cement + 3 Sand. $y =$	1 Cement + 5 Sand. $y =$
A	$1{,}877 + 206\sqrt{x-1}$	$953 + 299\sqrt{x-1}$	$469 + 299\sqrt{x-1}$
B	$953 + 227$	$313 + 248$	$199 + 199$
C	$1{,}991 + 490$	$1{,}038 + 313$	$740 + 305$
D	$1{,}770 + 327$	$427 + 412$	$284 + 313$
E	$1{,}592 + 341$	$668 + 320$	$356 + 263$
F	$2{,}404 + 299$	$825 + 334$	$540 + 284$
G	$1{,}582 + 270$	$782 + 270$	$483 + 249$
H	$1{,}436 + 334$	$711 + 270$	$341 + 256$
R	$2{,}631 + 441$	$1{,}507 + 341$	$995 + 341$
T	$1{,}920 + 455$	$1{,}024 + 313$	$626 + 227$
	Mean 339	Mean 312	Mean 274

Here the first constant, which can be obtained by tests lasting one week only, varies a good deal: but the second constant has no very great range of variation about its mean values. If the initial strength of the cement is known therefore, the strength at any age up to two years can be inferred with a certain degree of approximation.

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Other Cements.

Some experiments by Dr. Böhme on hydraulic lime have also been examined.[1] These are not very extensive, and they were carried to an age of thirteen weeks only. However, they agree well with a formula of the same general form with $n = 1$. The equation is therefore—

$$y = a + b\,(x - 1).$$

The following were deduced from the data—

Tension; 1 lime + 1 sand,

$$y = 20 + 11\,(x - 1).$$

Tension; 1 lime + 3 sand,

$$y = 31 + 17\,(x - 1).$$

Compression; 1 lime + 1 sand,

$$y = 97 + 44\,(x - 1).$$

Compression; 1 lime + 3 sand,

$$y = 122 + 22\,(x - 1).$$

The Paper is accompanied by several diagrams, from which the Figs. in the text have been engraved.

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[1] "Mittheilungen aus den k. technischen Versuchsanstalten zu Berlin," 1884, p. 46.

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(*Paper No. 2154.*)

"Investigation into the Strength of Steel and Wrought-Iron Girders."[1]

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Abstracted by WILLIAM ANDERSON, M. Inst. C.E., from a translation by H. SICCAMA, M. Inst. C.E.

THE girders experimented on were all beams with single-plate webs having flanges composed of angle-bars, and two or more layers of plates.

The Paper commences with some explanatory and introductory matter, and proceeds as follows:—

On the 23rd of February, 1877, in the works of the firm of Harkort, at Duisburg, Hochfeld, a riveted longitudinal beam was placed on two supports, and weighted on the centre until rupture took place. The beam rested near its extremities on wrought-iron rollers. The pressure on the centre of the top flange was applied by a lever 31 feet 2 inches long, with a ratio between the arms of 1 to 29. By means of a screw at the fulcrum, the lever could be kept in a horizontal position during the deflection of the girder. Lateral deflection was prevented by four angle-irons which supported the horizontal flanges sideways.

The scale-pan at the extremity of the lever was slowly and carefully loaded; the girder stood the test well until the calculated tension in the extreme lamina had reached 12·7 tons per square inch (20 kilograms per square millimetre). Up to that load the observed deflections were less than those calculated, but with the above strain they became equal to it. When the stress had reached the supposed limit of elasticity with a calculated tension of 15·9 tons per square inch (25 kilograms per square millimetre), a peculiar snap was heard, followed by a similar report a second later, after which the girder broke through the middle with

[1] The original appeared in the Tijdschrift van het Koninklijk Instituut van Ingenieurs, 12 Feb. 1884, and contains ten plates, besides numerous Tables. The MS. of Mr. Siccama's complete translation, with all the plates, diagrams, and tables, are in the Library of the Institution.

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a loud noise. The fracture showed a clean bright metallic surface, without any blemish which could explain the unexpected failure.

On the 19th and 20th of March, two longitudinal girders of the same form were tested in a similar way. To distribute the pressure more evenly, a cushion measuring 6·56 feet by 1 foot 2½ inches (2 metres by 370 millimetres), made of felt, oak, and iron-plates, was placed under the test-load on the girder. A snap was heard when the first of these girders was loaded to a calculated tension of 15·2 tons per square inch (24 kilograms per square millimetre). The lower flange of one of the bottom table angle-bars was fractured. No exceptional brittleness or hardness could be inferred from the appearance of the fracture.

The strength of the third girder proved to be even less. A calculated tension of 6·3 tons per square inch (10 kilograms per square millimetre), was successfully supported, but two reports were heard at a tension of 7·6 tons per square inch (12 kilograms per square millimetre). The deflection then was but small; the fracture could not easily be found, and the experiment was proceeded with. When the tension reached 11·7 tons per square inch (18·5 kilograms per square millimetre), another report was heard, and at 12·7 tons per square inch (20 kilograms per square millimetre), a rent was discovered in one of the lower angle-bars. A tension of 14·1 tons per square inch (22·2 kilograms per square millimetre), caused the lower flange-plate to tear with a report. At 17·8 tons per square inch (28 kilograms per square millimetre), the two lower angle-bars broke, and the bottom flange was ruptured in a second place at 2·62 feet (800 millimetres) distance from the first fracture. With 21·5 tons per square inch (33·9 kilograms per square millimetre) tension, the lower flange-plate and angle-bars were fractured for the third time. The girder, with its lower flange broken in several places, bore the slowly-augmenting load until a strain of 24 tons per square inch (37·8 kilograms per square millimetre) had been reached, when total fracture occurred. Immediately after this, parts of the broken girder near the fracture were drilled out and submitted to tests in compression and tension. These last gave satisfactory results on the whole, indicating an ultimate strength of 38 tons (60 kilograms) to 47·6 tons (75 kilograms per square millimetre) per square inch. The elongations amounted to a maximum of 21 per cent., and a minimum of 13·5 per cent., and the contraction of the sectional area varied between 26 and 41 per cent. Some of the rivet-heads were cut off to ascertain whether the rivet-holes had been properly filled. This proved to have been the case.

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From the 11th to the 14th of April, 1877, three steel longitudinal beams were tested at the Union Works, near Dortmund.

The beams were supported at their extremities on steel rollers, resting on piers of masonry, and were loaded by dead-weights laid on a platform suspended from the beams by four rods. In order to place the weight on the platform slowly and without shaking, the rails, which served as weights, were first laid over an iron beam placed on each side, and arranged to be lifted or lowered by means of screws. The lifting or lowering never exceeded a rate of $\frac{3}{4}$ inch per minute. Four angle-irons were fixed in order to prevent the platform swaying sideways. Lateral deflections of the tested beam were also guarded against by angle-irons fixed vertically. The deflection in the centre was observed on the bottom flange by direct measurements, and on the top flange by means of a special apparatus designed to magnify the readings.

The loading was regulated so as to cause calculated consecutive tensions in the extreme lamina of 6·3, 9·5, 11·4, 12·7, 15·2, 16·5 tons per square inch (10, 15, 18, 20, 24, 26 kilograms per square millimetre). After this tension had been reached, the stress was increased by 1·27 ton per square inch (2 kilograms per square millimetre) at a time, to 25·4 tons per square inch (40 kilograms per square millimetre), after which the gradual increase was at the rate of 0·63 ton per square inch (1 kilogram per square millimetre) at a time. Up to a tension of 17·8 tons per square inch (28 kilograms per square millimetre) the load was completely lifted off, after the deflection had been taken, by means of the screws. Above this limit the beam was totally unloaded at every 6·34 tons per square inch (10 kilograms per square millimetre) increase only, in order to save time. All weights causing a tension of over 19 tons per square inch (30 kilograms per square millimetre) were left on for some minutes before adding to them. The three girders tested stood a tension of 21·6 tons per square inch (34 kilograms per square millimetre), and carried a load of 57·7 tons (58,588 kilograms) without any unfavourable symptoms being observed. The deflections agreed pretty well with the calculations. With a tension of 21·6 tons per square inch (34 kilograms per square millimetre) on the first girder, a snap was heard, and a rent discovered on the lower flange-plate. With a tension of 24·1 tons per square inch (38 kilograms per square millimetre), the girder broke in two, the lower angle-bars being torn in two or three places.

The second girder tested stood a tension of 24·1 tons per square

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inch (38 kilograms per square millimetre). When the stress had increased to 25 tons per square inch (40 kilograms per square millimetre), the platform was found to be unevenly loaded. Before it could be lifted, previous to readjustment, a snap was heard, and a rent found in the lower angle-bars on the overweighted side of the platform. Reports were heard twice after resuming the experiment, and a second fracture was found in the lower angle-bars; the girder broke in two when the calculated tension had reached 27·9 tons per square inch (44 kilograms per square millimetre).

In testing the third girder, the first report was heard just before the tension of 24·1 tons per square inch (38 kilograms per square millimetre) was reached. No fracture could be found, but the complete fracture followed with a tension of 26 tons per square inch (41 kilograms per square millimetre).

The surfaces of the fractures were those characteristic of good ductible steel. Strips were drilled from near the fractures and tested for toughness and ductility. The strength amounted to 34·9 and 43·5 tons per square inch (55 to 69 kilograms per square millimetre), the elongation 7 to 19·5 per cent., contraction 12 to 45 per cent. on the sectional area.

The next beam, forming one of the cross-girders of a bridge, was consecutively weighted till 478 rails had been placed on the platforms, and a tension had been reached of 21·6 tons per square inch (34 kilograms per square millimetre), without any alarming symptom being observed; the actual surpassed the calculating deflection, however, to a considerable extent. As soon as the total load, amounting to 144·8 tons (147,034 kilograms), had been imposed, and the tension had attained 22·9 tons per square inch (36 kilograms per square millimetre), the girder broke in two with a loud report. A rent in a lower angle-bar, and a crack in the vertical web, were discovered. The rent of the web-plate showed a fine fibrous fracture. This was the first experiment in which a total fracture occurred before warning snaps or reports were heard.

The dimensions of the cross-girders had been calculated on the understanding that the extreme lamina were not to be strained beyond 6·34 tons per square inch (10 kilograms per square millimetre). The four steel girders, tested at Dortmund, broke when the calculated strain amounted to three and a half times this tension. The ultimate strength of the steel was found to be over 38·1 tons per square inch (60 kilograms per square millimetre). Whereas wrought-iron girders bear a load in the centre without breaking or showing signs of shearing in the rivets, till

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the calculated tension in the extreme lamina approaches the limit of absolute strength; riveted steel beams break when the calculated strain reaches only half that due to the direct tenacity of the steel.

The results of the steel tests thus proved very disappointing. The different authorities on iron and steel manufacture declared that they had encountered phenomena which they could not explain, and it was determined to extend the experiments by means of some rejected girders of larger dimensions, which could be cheaply obtained. These were made of soft steel, of hard steel, and of wrought-iron, and it was also determined to ascertain the difference between girders bolted and riveted together. Accordingly the following beams were experimented on: twenty longitudinal girders intended for the bridge across the River Waal at Nymegen. The component parts of three of these girders were annealed before riveting, while two of the girders were not riveted but bolted. Three cross-girders, three riveted girders of hard steel of the same dimensions as the longitudinals; three riveted girders of soft steel as above; three riveted iron girders as above.

For hard steel a minimum resistance against tension of 47·6 tons per square inch (75 kilograms per square millimetre) was prescribed, and an elongation on fracture of at least 14 per cent. The requirements for soft steel were a maximum resistance to tension of 31·7 tons per square inch (50 kilograms per square millimetre), and an elongation with fracture of not less than 25 per cent. During the investigation, the question arose as to whether it would be practicable to anneal the riveted steel girders without injuring them. From the steel-manufacturers in Westphalia no satisfactory answer could be obtained, consequently an investigation into this important matter became necessary, and one of the pieces of a girder broken in testing was used. It had a length of 13 feet 6 inches (4,128 millimetres), and was not much deformed. On the top and bottom flange-plates centre lines were marked, and the width was measured on each side of these lines at distances 10 inches (25 centimetres) apart. All rivets were proved by means of a hammer and found perfectly tight. The girder was then laid on its side in a roomy annealing furnace, which had been heated for thirty-six hours, and was considered to have attained a temperature of 1,112° Fahrenheit (600° C.). Care was taken to prevent sagging by packing with stones. The furnace was opened for about twenty minutes, while the girder was inserted and packed. To prevent unequal cooling the furnace was fired for five minutes after closing the doors, after which all openings were bricked up,

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and all joints and cracks were thoroughly luted with clay. These operations occupied forty-five minutes. The girder was left for sixty hours in the furnace, and was by that time completely cooled. It was then taken out and laid on the supports, on which it had rested during the marking of the centre lines. The girder proved to be warped over its full length; the top and bottom flange-plates were bent, and deviated respectively $20\frac{1}{8}$ inches (51 millimetres), and $5\frac{7}{8}$ inches (16 millimetres) from the original centre line. The web was buckled in several places. The rivets were again tested by the hammer and found tight. On cutting off one of the rivet-heads a slight layer of dust was found between the plate and the rivet-head. With other rivet-heads this phenomenon was not observed. The red lead, with which the girder had been painted, was partly burned, and partly remained in flakes adhering to the metal. The deformations were so great and numerous that it was considered as demonstrated that, even if fixed or strutted during annealing, no satisfactory results could be expected.

The results of the investigations are collected in Tables, and represented graphically.

It will be seen, with regard to the wrought-iron girders, that the deflection increased in direct proportion to the load, till a tension in the extreme lamina of 16·6 to 17·1 tons per square inch (26 to 27 kilograms per square millimetre) was reached. This rate of deflection did not cease when the girder was again submitted to test, after having been previously loaded up to a tension of 19 tons per square inch (30 kilograms per square millimetre), and then unloaded, and a permanent set was observed of $\frac{5}{176}$ inch (4·18 millimetre). Up to this limit the deflections of the three girders agree very closely. At 17·1 tons per square inch (27 kilograms per square millimetre) the deflections were equal to those calculated, but beyond this limit the first exceeded the last in increasing measure. At 11·4 tons per square inch (18 kilograms per square millimetre) a slight permanent set (0·3 millimetre) was observed; this increased a little (to 1·08 millimetre), with a tension of 15·9 tons per square inch (25 kilograms per square millimetre), to $\frac{5}{176}$ inch (4·18 millimetres), with a tension of 19 tons per square inch (30 kilograms per square millimetre), and to $\frac{3}{8}$ inch (10 millimetres), and $1\frac{5}{176}$ inch (49 millimetres) with 24·12 tons per square inch (38 kilograms per square millimetre). The three girders resisted perfectly till a tension of 19·7 tons per square inch (31 kilograms per square millimetre) was imposed, but soon after some rivet-heads in the middle of the girders

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began to manifest displacement, which increased with the tension, the rivets gradually cutting into the flange-plates and webs. With 22·2 tons per square inch (35 kilograms tension per square millimetre) the compressed bottom flange,[1] which was supported laterally by the vertical side-stays, began to deflect laterally between the stays. In addition, the beginning of a rent was observed in two rivet-holes in the top flange-plate of one of the girders. With 24·1 tons per square inch tension (38 kilograms per square millimetre), the limit of strength seems to have been reached. The little rent before mentioned was followed by a similar one in an adjacent rivet-hole of the flange-plate. When 24·6 tons per square inch (39 kilograms per square millimetre) tension was attained failure commenced, and a grinding rustling sound was heard repeatedly. At first the upper flange-plate opened, and was followed a few moments after by the second and third; the two angle-bars broke next, two dull reports were heard, the web-plate cracked, the rent widening slowly to $\frac{1}{16}$ inch (2 millimetres).

With a tension of 24·1 tons per square inch (38 kilograms per square millimetre), the three girders were visibly deformed. The top and bottom flanges showed lateral deflections. The plates of the bottom flanges between the rows of rivets were buckled vertically; the web-plates were bulged in many places. The pressure having been continued, the girder lost its symmetry, its resistance was destroyed, and the pressure-gauge indicated a continuously diminishing tension. The deflection did not decrease with the removal of the load. The top and bottom flanges showed considerable elongation and compression, amounting, in a length of four rows of rivets, or 1 foot (320 millimetres) to $\frac{7}{16}$ inch (11 millimetres) and $\frac{1}{8}$ inch (3 millimetres) respectively. Two vertical angle-bars secured to the web at a distance of 2·9 feet from each other (40 millimetres), receded from each other at the top $\frac{11}{16}$ inch (18 millimetres), and approached each other the same distance at the bottom.

Bending and tensile tests made on the several parts of the girders show a resistance against tension of 24·1 tons, and 25·4 tons per square inch (38 to 40 kilograms per square millimetre), an elongation at the point of fracture of 12 to 25 per cent., and a contraction of fractured area of 11 to 35 per cent.

Not a single unfavourable phenomenon was observed during

[1] Many of the girders were tested by hydraulic pressure applied upwards in the middle, hence the bottom flanges were often in compression.

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these experiments on wrought-iron girders. The results corroborate sufficiently the accuracy of the calculations usual for determining their strength and deflection; they show that the admitted maximum tension gives a factor of safety of 5, that wrought-iron endures without detriment, shaping, drilling and riveting, and that, in being built up in the usual way, it makes a girder which leaves nothing to be desired in respect of strength and flexibility.

The steel girders, ranging in length from 17·6 feet (5,359 millimetres) to 20·3 feet (6,187 millimetres) show with tensions of 3·2 tons, 6·3 tons, 9·5 tons, 12·7 tons per square inch (per square millimetre of 5, 10, 15 and 20 kilograms) very similar deflections; the extreme variation amounted to only $\frac{1}{32}$ inch (0·8 millimetre) in one case. After unloading, the deflections nearly disappeared. In the case of three girders, 27·9 feet (8,500 millimetres) long, the deflections at the above indicated tensions showed variations ranging to $\frac{1}{8}$ inch (3·03 millimetres). The permanent sets, after a tension of 12·7 tons per square inch (20 kilograms per square millimetre) amounted to $\frac{1}{32}$ inch (1·01 millimetre). Also, with a tension of 13·3 tons per square inch (21 kilograms per square millimetre) in eighteen steel girders, the ratio between deflection and load remain constant, and the top flange-plate of one girder was torn with a tension of 11·4 tons per square inch (18 kilograms per square millimetre). Under a tension of 14 tons per square inch (22 kilograms per square millimetre) a snap was heard, but no crack was found. In another case one top angle-bar was completely, and the top flange-plate partly, torn under a tension of 13·6 tons per square inch (20 kilograms per square millimetre). Again, a tension of 15·2 tons per square inch (24 kilograms per square millimetre) was sufficient to destroy the top flange-plate and two angle-bars of another girder. On reaching the supposed limit of elasticity, corresponding to 16·5 tons per square inch (26 kilograms per square millimetre) in the case of another beam, a report was heard, and the top flange-plate showed a tendency to move over the plate just underneath it.

After unloading, all the girders showed a small permanent set (varying from 0·06 millimetre to 1·41 millimetre). The deflections observed were, as a rule, smaller than those calculated in the fifteen lighter girders, but those in the three heavy girders were greater. No unfavourable symptoms were observed with a tension of 17·1 tons per square inch (27 kilograms per square millimetre), but under calculated tensions of 17·8 tons, 18·4 tons, and 19 tons, up to 31·1 tons per square inch (28, 29, 30, &c., up to 49 kilograms

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per square millimetre) one or other of the girders was seen to be partly or wholly torn. The weakest girder succumbed at a tension of 20·3 tons per square inch (32 kilograms per square millimetre). Seven girders broke below 25·4 tons per square inch (40 kilograms per square millimetre); ten girders failed while the calculating strains were between 25·4 tons per square inch and 31·7 tons per square inch (40 kilograms and 50 kilograms per square millimetre). One single girder answered expectations, and showed no fissures or rents before the calculated strain in the extreme lamina amounted to 37·5 tons per square inch (59 kilograms per square millimetre). From each of the examined girders strips were taken and tested by bending and stretching. On the whole, the results were satisfactory. The resistance of tension in the direction of the fibres amounted to a maximum of 54·6 tons per square inch (86 kilograms per square millimetre), and to a minimum of 24·6 tons per square inch (39 kilograms per square millimetre). The greatest and smallest elongation observed were 25 and 5 per cent. The contraction on the sectional area varied from 49 to 11 per cent. Also the plates and bars bent cold gave satisfactory results.

The two experiments on steel girders put together with bolts contribute valuable information about the usual joining and riveting. The flange-plates, angle-bars, and web-plates of these girders were placed in position, and sufficiently held together by a few bolts to enable the rivet-holes to be drilled right through the several thicknesses of metal. All stretching and compression was carefully avoided. The bolt-holes were drilled slightly conical and were fitted with bolts of the same shape. Washers were placed under the nuts, which were screwed home without any special strain. The two girders, under slowly-increasing loads, showed regular deflections a little less than those calculated, and the deflections agreed well with those of the other steel girders, until a calculated strain of about 19 tons per square inch (30 kilograms per square millimetre) was reached. With continued increase of load, the deflections increased at a higher rate, and with a tension of 22·9 tons per square inch (36 kilograms per square millimetre) the values exceeded those calculated. No snaps or rents were observed. The bottom flange of one of the girders showed incipient lateral deflection with a tension of 22·9 tons per square inch (36 kilograms per square millimetre), and this deformation slowly increased. The second girder, which was of greater length, remained longer in good condition, and only lost shape under a tension of 31·7 tons per square inch (50 kilograms per square millimetre). The defor-

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mations were greater and more irregular than those of the iron girders described before. Whether these more favourable results were owing to bolting instead of riveting cannot be decided with certainty. The results of the following six experiments seem to demonstrate that riveting does not necessarily cause harm. Six Bessemer steel bars $\frac{3}{16}$ inch (49 millimetres) thick and $2\frac{9}{16}$ inch (6·8 millimetres) wide were drilled through in the middle with two holes $\frac{13}{16}$-inch (21 millimetres in diameter). One of the holes was then filled with a steel rivet in the usual way. The bars were then tested for tension, and they all broke through the hole not riveted with a tension of 39·4 tons to 40 tons per square inch (62 to 63 kilograms per square millimetre). In the vicinity of the bolts not a vestige of splitting or rending could be found.

The annealing of the different parts of the girders before riveting did not answer expectations, the tests being, in fact, the least satisfactory. The top cover-plate and angle-bars of the three girders were torn with a tension of but 9·5 tons, 10·8 tons, and 12·7 tons per square inch (15 kilograms, 17 kilograms, and 20 kilograms per square millimetre). The tension of 3·2 tons per square inch (5 kilograms per square millimetre), had barely been reached in one of the girders when the deflection exceeded the calculated quantity. In the two other girders, this took place with a tension of about 14 tons per square inch (22 kilograms per square millimetre). The girders broke with a calculated tension of 14·6 tons, 16·5 tons, and 17·8 tons per square inch (23 kilograms, 26 kilograms, and 28 kilograms per square millimetre), after several premonitory signs of failure. No explanation has been given for these exceptionally unfavourable results. The annealing was performed in the usual manner and with all care. The tests were made during summer. The steel was of ordinary quality. The strips taken from the web-plates of the broken girders after the experiments gave, on being tested, an ultimate strength of 32·4 tons and 33·0 tons per square inch (51 kilograms, 52 kilograms per square millimetre), those taken from the cover-plates resisted a tensile strain of 29·2 tons, 31·7 tons, and 34·9 tons per square inch (46 kilograms, 50 and 55 kilograms per square millimetre). The elongations before fracture varied between 8 and 21 per cent. with one exception, when a cover-plate seemed to have been burned. The contraction on the sectional area amounted from 27 to 41 per cent. The metal was generally granular on the fractured surface, but could be bent cold in a satisfactory manner.

The experiments on the three hard steel girders gave surpris-

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ingly favourable results. The deflections agreed completely, and increased uniformly with the load, remaining always a little below that calculated until an estimated strain of 34·9 tons per square inch (55 kilograms per square millimetre) was reached in the extreme fibres, and with this tension the two upper angle-bars of one of the girders broke. With an increased load, this beam broke at a tension of 43·2 tons per square inch (68 kilograms per square millimetre). The second girder tore, without previous damage, when the calculated strain amounted to 37·5 tons per square inch (59 kilograms per square millimetre). It is remarkable that the top flange-plate showed a rent of $\frac{5}{16}$ inch (8 millimetres) wide, while in the web-plate only a small fissure could be found near one of the rivet-holes. In these three girders no motion of the rivets or cutting into the plates whatever took place. The third girder withstood a tension of 43·2 tons per square inch (68 kilograms per square millimetre); the experiment had then to be stopped owing to signs of warping.

Strips taken from these three hard steel girders gave a resistance against tension of from 51·4 tons to 54·6 tons per square inch (81 kilograms to 86 kilograms per square millimetre), an elongation before fracture of from 9 to 15 per cent., a contraction of from 24 to 36 per cent.

It must be borne in mind that the steel in these girders was made purposely for the experiment, that great trouble was experienced in the manufacture before the material satisfied the different requirements, and that, therefore, it cannot be considered as a fair trading specimen, nor does it compare favourably with wrought-iron. When the wrought-iron girders were loaded till the strain in the extreme lamina amounted to 86 per cent. of the ultimate strength, or to 80 per cent. of the tensional resistance, deformation, indicating the approach of the limit of endurance, began to show, and failure did not occur suddenly, but by gradual yielding.

In the steel girders, fracture occurred suddenly, when the strains reached 73 per cent. of the ultimate strength and 66 per cent. of the extreme resistances to tension. The power of resistance beyond this point was of small value.

The deflection of the iron and the hard steel girders was equal until a strain of 16·5 tons per square inch (26 kilograms per square millimetre) was reached. Above this limit the deflection in the iron girders was the greater.

On the experiments on three soft steel girders, the following remarks may be made. The girders show deflections uniformly

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increasing with the load, until a strain of 12·7 tons per square inch (20 kilograms per square millimetre) was reached. After unloading, the permanent set was very slight (from 0·66 millimetre to 0·76 millimetre). After 14 tons per square inch (22 kilograms per square millimetre), the observed deflections exceeded those calculated, and these differences increased rapidly with the load. With a strain of 17·8 tons to 18·4 tons per square inch (28 and 29 kilograms per square millimetre), local bulges and deformations began to show, and some displacement of rivet-heads was noticed. A considerable deformation of the bottom flanges and bulges in the web-plates followed shortly after. When a calculated strain of 22·2 tons per square inch (35 kilograms per square millimetre) had been reached, the girders were considerably deformed, and the readings of the hydraulic gauge indicated the collapse of the girders.

Test samples showed a resistance to tensional strain of from 26·7 tons to 31·1 tons per square inch (42 kilograms to 49 kilograms per square millimetre), an elongation of from 12 to 24 per cent., and a contraction on the sectional area of from 42 to 50 per cent.

Though these results are not unfavourable they are not so good as those obtained from iron girders. The deflections of the steel girders were greater, the deformations and bulgings originated at an earlier period and were more irregular. The material for these girders was also manufactured specially for these experiments.

[NOTE.—It cannot fail to strike the reader of the above abstract that the quality of steel used in the experiments was very irregular, and quite different from steel used for structural purposes at the present time in this and other countries.—W. A.]

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(*Paper No. 2163.*)

"Experiments on the Discharge of Water of Different Temperatures."

By JOHN GEORGE MAIR, M. Inst. C.E.

IN testing engines the most important measurements that require to be made are, the power, the boiler-pressure, the feed and its temperature, as with these data the amount of heat used can be determined; but where possible it is always advisable to check the results so obtained by the amount of rejected heat.[1] To do this it is necessary to ascertain the volume of injection water, and its rise in temperature after it has passed through the air-pump; but as the quantity used for injection is about thirty times the weight of feed, it is not convenient to measure such a large volume by direct weighing. Water-meters, unfortunately, are not sufficiently accurate, and the readiest way to measure the injection or air-pump discharge is by passing it over a tumbling-bay or letting it flow through an orifice.

The Author on various engine trials[2] has preferred to measure the air-pump discharge through a circular orifice, with a head of about 2 feet over the centre, in place of using a tumbing-bay, as the head is more easily measured, and any slight error in observing it does not so seriously influence the quantity of water delivered. Many experiments were made by the Author, but those were only applicable to a tank of similar size and shape to that used on the trials, and where the orifice is placed in the same relative position to the sides and bottom. The coefficient on those experiments was found to average 0·618, but as the edges of the orifices were so close to the bottom and sides, imperfect contraction must have taken place, so that it seems apparent that the coefficient was too high to be generally used except under the special circumstances mentioned above. Many careful experimenters have tested the discharge from circular orifices, and the most important results are given below:—

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[1] Minutes of Proceedings Inst. C.E. vol. lxx. p. 315. "Mair on Independent Testing of Steam-Engines."

[2] *Ibid.* vol. lxx. p. 320.

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TABLE I.

Experimenter.	Head.	Diameter of Orifice.	Coefficient.
	Feet.	Inch.	
Eytelwein	2·4	1·0	0·618
Bossut	0·6	1·0	0·649
Castel	2·7	1·2	0·629
Venturi	2·9	1·6	0·622
Rennie	1·0	1·0	0·633
„	2·0	1·0	0·619
Weisbach	2·0	1·2	0·614
„	2·0	1·6	0·607
„	0·8	1·2	0·622
„	0·8	1·6	0·614

These results vary so much that Mr. James Simpson, M. Inst. C.E., and the Author decided to make a complete set of experiments, with a view to ascertain what the coefficients should be for different diameters of orifices and underheads suitable for the purposes of engine-testing, and if the variation in temperature affected the coefficient of discharge.

To carry out the experiments on a sufficiently large scale a cast-iron tank to contain 512 cubic feet was made, to measure the water in. The sides were carefully moulded and the joints planed, that the tank might be accurately square. Glass strips were fixed in the sides so that the level of water could be seen, and the tank was gauged first by measurement, then by weighing in small tanks of water at a known temperature, and after that by filling it with water from a 10-gallon standard measure, kindly lent for the purpose by Messrs. Donkin. This can was mounted on a frame and automatically registered the number of times it was emptied. Different assistants measured the contents of the tank, and the sides were marked every 20 cubic feet from the mean of all the readings; it was then finally checked by Mr. James Simpson with the 10-gallon measure, and the markings were found perfectly correct, so that the results given in this Paper may be confidently relied on.

A valve was placed on the bottom of the measuring-tank to control the water run out into the discharge-tank where the orifices were fixed, and after a little practice it was found that it could be so regulated that a constant head could be steadily maintained over the orifice.

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Three assistants were required to carry out the experiments; one to measure the fall of water-level in the measuring-tank and the time it took to fall, another to regulate the valve, and the third to measure the head over the centre of the orifice. All observations were taken every fifteen seconds. Each experiment was made by running about 8 tons of water out of the tank, and with the smaller orifices this quantity took a considerable time to discharge, so that even if any slight error existed in the estimation of the time (which was taken with a minute-and-second chronograph) when the water was at any particular level, it could not materially affect the results. Plate 6, Fig. 1, shows the arrangement adopted, **A** being the measuring-tank, **B** the valve, and **C** the discharge-tank. Several baffle-plates were fixed in the latter tank, causing the water to flow uniformly and slowly to the orifice. The head over the centre was measured in a glass tube $1\frac{1}{2}$ inch in diameter, to avoid as far as possible any effect due to capillarity. Nine gun-metal plates were made, faced true on their sides, and bored out to gauge in the lathe, the orifices being from 1 inch increasing by $\frac{1}{4}$ inch to 3 inches in diameter, and after a large number of preliminary trials to enable the assistants in charge to get accustomed to their duties, a set of about three hundred experiments were carried out, and the coefficients calculated. It was found, however, that, although for each diameter of orifice the coefficients were very regular, for the different heads experimented with, no regularity existed between the coefficients for each sized plate, and for some time it could not be ascertained why this difference existed. Ultimately one of the plates was tried by letting the water flow out from one side, and then reversing the plate and letting the water flow from the other edge, when a difference of about 20 per cent. was noted between the coefficients for the two edges. On examination the hole in the plate was found perfectly square, parallel and exact to size, but a hardly perceptible rounding of the edge was noticed on that side that gave the highest coefficient.

The plates were next got up with scrapers to a sharp square edge, and the diameters measured with a Brown and Sharp vernier-gauge, the areas calculated from the mean diameters being given in the Tables. An entirely fresh series of experiments was then made, and the mean of the results is given in Table II.

It will be seen that the coefficients are fairly regular in falling as the head is increased, and also for the same head as the diameter of the orifice is increased, except the results obtained from three plates which appear low. The $1\frac{3}{4}$-inch plate was examined, turned

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TABLE II.—COEFFICIENTS of DISCHARGE FROM CIRCULAR ORIFICES.

Temperature 51° to 55°.

Head. Inches.	Approximate Diameter of Orifice in Inches. 1	1¼	1½	1¾	2	2¼	2½	2¾	3
	Absolute Area in Square Feet.								
	0·00546	0·00852	0·012281	0·016749	0·021806	0·027576	0·033898	0·040933	0·049139
	Coefficients.								
9	0·616	0·614	0·616	0·610	0·616	0·612	0·607	0·607	0·609
12	0·618	0·612	0·612	0·611	0·612	0·611	0·604	0·608	0·609
15	0·618	0·614	0·610	0·608	0·612	0·608	0·605	0·605	0·606
18	0·610	0·612	0·611	0·606	0·610	0·607	0·603	0·607	0·605
21	0·612	0·611	0·611	0·605	0·611	0·605	0·604	0·607	0·605
24	0·609	0·618	0·609	0·606	0·609	0·606	0·604	0·604	0·605

round, re-checked for diameter of orifice, but no alteration in the coefficient was produced; and the Author is unable to give a reason why the coefficients of these plates are so low; possibly a slight burr was on their edges, but it could not be detected, and re-scraping up the sides made no practical difference in the discharge.

It will be noted how much lower the coefficients are than those given by previous experimenters, and in all probability the cause is due to a slight difference in the edges of the orifices so largely affecting the results, and this also probably accounts for the discrepancies that exist between the coefficients given by the various observers who from time to time have experimented on the discharge through orifices.

Professor Unwin analyzed the results in Table III., and found that within limits, the coefficient of discharge is closely approximated by the following formula:—

$$(1.) \qquad C = 0{\cdot}6075 + \frac{0{\cdot}0098}{\sqrt{h}} - 0{\cdot}0037\, d,$$

where h is the head in feet, d the diameter in inches.

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EFFECT OF TEMPERATURE ON SHARP-EDGED AND CONOIDAL ORIFICES.

In the Journal of the Franklin Institute for May 1878,[1] Chief-Engineer Isherwood, of the United States Navy, gives an account of a series of experiments made on the discharges of water from orifices at different temperatures, and leading to the conclusion that temperature has a very marked influence on the discharge, in fact that the velocity of discharge increases 12 per cent. as the temperature rises from 32° to 212°, and 8 per cent. from 60° to 180°.

Professor Unwin made a series of experiments, published in the "Philosophical Magazine," for October 1878,[2] and the results he obtained show that in the case of a thin-edged orifice the temperature has an extremely small influence on the discharge, and that unless the small differences in the experiments are errors of observation, the discharge is slightly greater at low temperatures. These experiments were, however, made on a limited scale, and for the small variations of temperature, say from 70° to 120°, that are likely to occur in the air-pump discharge, it is not probable the coefficient would vary; but as a good opportunity was afforded to the Author of making experiments on a larger scale, a steam-pipe was laid into the tanks to heat the water, and a series of experiments was carried out.

TABLE III.—SHARP-EDGED ORIFICE. 2½ inches in diameter.

Head.	Temperature.	Coefficient.
Inches.	°	
21	57	0·604
,,	92	0·604
,,	110	0·604
,,	153·5	0·607
,,	179	0·607

Table III. is made by grouping together those experiments carried out at different temperatures.

From these experiments, which were conducted with great care, some 8 or 9 tons of water being run in each case, it appears that there is a very slight increase in the discharge, as the temperature rises, but not sufficient to be of any practical account.

Professor Unwin then suggested that a further series of experi-

[1] Third Series, vol. lxxv. p. 339.

[2] Fifth Series, vol. vi. p. 281.

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ments should be carried out with a conoidal orifice, made in the form given in his article on "Hydro-Mechanics," "Encyclopædia Britannica," p. 463, and those results are grouped together and given in Table IV.

TABLE IV.—CONOIDAL ORIFICE. 1½ inch in diameter.

Head.	Temperature.	Coefficient of Velocity C_v.	Coefficient of Resistance by Formula (2).	Coefficient of Resistance by Formula (3).
Inches.	°			
21	55·0	0·961	0·0828	0·0825
,,	99·5	0·965	0·0739	0·0736
,,	110·1	0·967	0·0694	0·0698
,,	119·0	0·9685	0·0661	0·0664
,,	170·0	0·981	0·0391	0·0390

With a conoidal orifice there is no contraction of the jet, and as the coefficient is one of velocity only, it is convenient to reduce it to a coefficient of resistance by the formula given in the article "Hydro-Mechanics," "Encyclopædia Britannica," p. 462, C_r being the coefficient of resistance, and C_v the coefficient of velocity,

$$(2.) \qquad C_r = \frac{1}{C_v^2} - 1;$$

and calculating out the results of the experiments by the method of least squares, the following formula for the coefficient of resistance is obtained:—

$$(3.) \quad C_r = 0{\cdot}0799 + 0{\cdot}000184\, t - 0{\cdot}0000025\, t^2.$$

THE EFFECT OF TEMPERATURE ON THE DISCHARGE THROUGH PIPES.

A Paper by Professor Unwin on "Experiments on the Friction of Disks Rotated in Fluid," has been published in the Minutes of Proceedings of the Institution,[1] wherein it is shown that a marked diminution of friction was found as the temperature increased, and it is suggested "that a correction for temperature ought to be introduced in experiments on the flow of water in pipes and channels." This subject has been treated by many observers, but principally with small tubes, and an increase in the discharge has been generally noted as the temperature rises. The Author, wishing to verify this result on a larger scale, fitted

[1] Vol. lxxx. p. 221.

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directly into the measuring tank a 1½-inch brass tube, 25 feet long, which was made perfectly smooth inside and to gauge (as a mandril was driven through it), and in order to avoid the loss at entry the head was read from a manometer fixed 12 inches from the side of the tank, so that the net length of the pipe was 24 feet. The head decreased as the level of the water fell in the tank; corrections therefore had to be made to get the mean head and velocity. Numerous experiments were made which have been grouped together, and are given in Table V.

TABLE V.

Head.	Velocity.	ζ.	m.
	56½°–57°		
3·783	8·43	0·00441	0·000276
3·388	7·92	0·00447	0·000277
2·798	7·13	0·00456	0·000276
2·399	6·53	0·00466	0·000277
2·167	6·21	0·00466	0·000274
1·790	5·50	0·00490	0·000280
1·623	5·28	0·00482	0·000274
1·056	4·12	0·00515	0·000279
	70°		
3·699	8·56	0·00418	0·000263
3·323	8·10	0·00419	0·000260
2·744	7·19	0·00440	0·000266
2·354	6·64	0·00442	0·000264
1·760	5·67	0·00453	0·000262
	80°		
3·695	8·67	0·00407	0·000256
3·312	8·14	0·00414	0·000257
2·733	7·32	0·00422	0·000257
2·339	6·73	0·00428	0·000256
1·745	5·68	0·00448	0·000259
	90°		
3·258	8·22	0·00399	0·000249
2·679	7·38	0·00407	0·000248
2·518	7·12	0·00412	0·000249
2·321	6·80	0·00416	0·000249
2·094	6·40	0·00424	0·000251
1·716	5·74	0·00431	0·000250
1·349	5·00	0·00447	0·000251
	100°		
3·619	8·79	0·00388	0·000245
3·238	8·29	0·00390	0·000243
2·674	7·42	0·00402	0·000245
1·710	5·79	0·00422	0·000245
	110°		
3·539	8·86	0·00373	0·000236
3·179	8·40	0·00373	0·000233
2·626	7·50	0·00387	0·000236
2·245	6·90	0·00390	0·000235
1·686	5·85	0·00408	0·000237
	120°		
3·154	8·42	0·00368	0·000231
2·674	7·69	0·00375	0·000230
2·230	6·98	0·00379	0·000228
2·001	6·53	0·00389	0·000228
1·722	6·03	0·00392	0·000229
1·295	5·12	0·00409	0·000231
	130°		
3·480	9·03	0·00353	0·000224
3·124	8·51	0·00357	0·000224
2·585	7·62	0·00369	0·000226
2·221	7·02	0·00373	0·000225
1·662	5·95	0·00389	0·000227
	160°		
1·818	6·58	0·00348	0·000207
1·637	6·80	0·00355	0·000209
1·360	5·57	0·00363	0·000209
1·170	5·17	0·00364	0·000205
0·884	4·39	0·00380	0·000208

The ordinary formula for the flow of water in pipes is $h = \zeta \frac{4L}{D} \cdot \frac{V^2}{2g}$, where ζ is the coefficient of friction.

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It will be seen from the Table that the value of ζ diminishes as the temperature is increased and also as the velocity is decreased; it is therefore more convenient for ascertaining the effect of temperature, to use the formula given by Professor Osborne Reynolds in his Paper entitled, "An Experimental Investigation of the circumstances which determine whether the motion of water shall be direct or sinuous, and of the Law of Resistance in Parallel Channels," communicated to the Royal Society in 1883,[1] as in that formula the constant is independent of the velocity for a pipe of given length and diameter, and of the same nature in its internal surface.

This formula is given in a simplified form in "The Engineer" of January 1st, 1886, p. 1,

$$h = m \, L \, \frac{V^n}{d^{1-n}},^2$$

where m is the constant just referred to, and the index n is dependent on the internal surface of the pipe, ranging in the experiments made by Professor Reynolds from 1·7 for the smoothest to 2·0 for the roughest surfaces. Professor Reynolds gives a very ready graphic manner of obtaining n from any series of experiments by plotting the results in a logarithmic homologue, that is, with $\log. \frac{h}{L}$ as abscissas and log. V as the ordinates, Plate 6, Fig. 2.

It will be noted how well the experiments at each temperature fall in parallel straight lines, showing that the index n is independent both of the temperature and of the velocity; for the special pipe experimented on by the Author, $n = 1{\cdot}795$. The value of the constant m has been calculated and given in Table V. It varies almost directly as the temperatures, the mean being:—

Fahrenheit. °	m.
57	0·000276
70	0·000263
80	0·000257
90	0·000250
100	0·000244
110	0·000235
120	0·000229
130	0·000225
160	0·000206

Temperature, therefore, had a very marked influence on the discharge through the pipe, as the loss or frictional head is about

[1] Proceedings of the Royal Society of London, vol. xxxv. p. 84.

[2] See also "Applied Mechanics," J. H. Cotterill, p. 578. Macmillan and Co.

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25 per cent. less at 160° than at 57°. Using the same empirical formula as was adopted with the experiments on the friction of disks[1] where $c_t = 0{\cdot}01328\ (1 - 0{\cdot}0021\ t)$, and calculating the values of the constants m in the above Table by the method of least squares, it will be found that $m = 0{\cdot}000311\ (1 - 0{\cdot}00215\ t)$, so that the effect of temperature on the flow through the pipe with which the Author experimented was practically the same as in the disk experiments which were made between the limits of 41° and 130°. The Author regrets his inability to make a more extended series of experiments, so as to throw more light on the laws which govern the flow of water through pipes at different temperatures.

In conclusion he wishes to express his thanks to Professor Unwin, M. Inst. C.E., for his great assistance, and also to Messrs. Homersham, Drew and Tyacke, Studs. Inst. C.E., for the careful manner in which they recorded the details of the experiments.

This Paper is illustrated by two diagrams, from which Plate 6 has been prepared.

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[1] Minutes of Proceedings, Inst. C.E. vol. lxxx. p. 227; where at line eight from the bottom of the page a — sign should be substituted for the + sign, and the formula should read $c_t = 0{\cdot}1328\ (1 - 0{\cdot}0021\ t)$.

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APPENDIX.

Copies of the figures as taken by the observers. The experiments with the orifices were all made in a similar manner, and the readings for one trial are sufficient to show the character of the experiments.

DISCHARGE THROUGH A 2-INCH CIRCULAR ORIFICE.

Time.	Head in Gauge on Tank C.	Time.	Head in Gauge on Tank C.	Time.	Head in Gauge on Tank C.	Time.	Head in Gauge on Tank C.	Time.	Head in Gauge on Tank C.
M. S.	Foot.	M. S.	Foot.	M. S.	Foot.	M. S.	Foot.	M. S.	Foot.
0 0	1·250	10 15	1·250	20 30	1·250	30 45	1·250	41 0	1·250
0 15	1·260	10 30	1·250	20 45	1·250	31 0	1·250	41 15	1·250
0 30	1·260	10 45	1·250	21 0	1·250	31 15	1·250	41 30	1·250
0 45	1·255	11 0	1·250	21 15	1·250	31 30	1·250	41 45	1·250
1 0	1·250	11 15	1·250	21 30	1·250	31 45	1·250	42 0	1·250
1 15	1·250	11 30	1·250	21 45	1·250	32 0	1·250	42 15	1·250
1 30	1·250	11 45	1·250	22 0	1·250	32 15	1·250	42 30	1·255
1 45	1·255	12 0	1·250	22 15	1·250	32 30	1·250	42 45	1·250
2 0	1 250	12 15	1·250	22 30	1·250	32 45	1·250	43 0	1·250
2 15	1·240	12 30	1·250	22 45	1·250	33 0	1·250	43 15	1·250
2 30	1·250	12 45	1·250	23 0	1·250	33 15	1·250	43 30	1·250
2 45	1·250	13 0	1·255	23 15	1·250	33 30	1·250	43 45	1·250
3 0	1·250	13 15	1·250	23 30	1·250	33 45	1·250	44 0	1·250
3 15	1·245	13 30	1·250	23 45	1·250	34 0	1·250	44 15	1·250
3 30	1·250	13 45	1·255	24 0	1·250	34 15	1·260	44 30	1·250
3 45	1·250	14 0	1·250	24 15	1·260	34 30	1·260	44 45	1·250
4 0	1·250	14 15	1·250	24 30	1·255	34 45	1·255	45 0	1·250
4 15	1·245	14 30	1·250	24 45	1·250	35 0	1·250	45 15	1·255
4 30	1·250	14 45	1·250	25 0	1·250	35 15	1·250	45 30	1·250
4 45	1·250	15 0	1·250	25 15	1·250	35 30	1·245	45 45	1·250
5 0	1·250	15 15	1·250	25 30	1·250	35 45	1·250	46 0	1·250
5 15	1·250	15 30	1·250	25 45	1·250	36 0	1·250	46 15	1·250
5 30	1·250	15 45	1·250	26 0	1·250	36 15	1·250	46 30	1·250
5 45	1·250	16 0	1·250	26 15	1·250	36 30	1·250	46 45	1·250
6 0	1·250	16 15	1·250	26 30	1·250	36 45	1·250	47 0	1·250
6 15	1·250	16 30	1·250	26 45	1·250	37 0	1·250	47 15	1·250
6 30	1·250	16 45	1·250	27 0	1·250	37 15	1·250	47 30	1·250
6 45	1·250	17 0	1·250	27 15	1·250	37 30	1·250	47 45	1·250
7 0	1·255	17 15	1·250	27 30	1·250	37 45	1·255	48 0	1·250
7 15	1·250	17 30	1·250	27 45	1·250	38 0	1·250	48 15	1·250
7 30	1·250	17 45	1·250	28 0	1·250	38 15	1·250	48 30	1·250
7 45	1·250	18 0	1·250	28 15	1·250	38 30	1·250	48 45	1·250
8 0	1·250	18 15	1·250	28 30	1·250	38 45	1·250	49 0	1·250
8 15	1·250	18 30	1·250	28 45	1·250	39 0	1·250	49 15	1·250
8 30	1·250	18 45	1·250	29 0	1·250	39 15	1·250	49 30	1·245
8 45	1·255	19 0	1·250	29 15	1·250	39 30	1·250	49 45	1·250
9 0	1·255	19 15	1·250	29 30	1·250	39 45	1·250	50 0	1·250
9 15	1·255	19 30	1·250	29 45	1·250	40 0	1·250	50 55	
9 30	1·255	19 45	1·250	30 0	1·250	40 15	1·250		
9 45	1·250	20 0	1·250	30 15	1·250	40 30	1·255	360 cubic feet discharged from Tank A.	
10 0	1·250	20 15	1·250	30 30	1·250	40 45	1·255		

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DISCHARGE THROUGH 1½-INCH BRASS TUBE.

Quantity of Water in Measuring Tank.	Temperature 56½°.		Temperature 130°.	
	Head in Manometer.	Time.	Head in Manometer.	Time.
Cubic Feet.	Feet.	Min. Sec.	Feet.	Min. Sec.
500	4·29	..	3·94	..
480	4·09	3 11	3·77	2 58
460	3·89	6 21	3·58	5 59
440	3·70	9 39	3·42	9 3
420	3·51	13 2	3·24	12 9
400	3·31	16 30	3·05	15 23
380	3·12	20 5	2·89	18 48
360	2·92	23 53	2·70	22 18
340	2·72	27 47	2·52	25 55
320	2·53	31 46	2·34	29 39
300	2·33	36 0	2·16	33 38
280	2·12	40 28	1·96	37 46
260	1·93	45 9	1·79	42 5
240	1·73	50 11	1·59	46 47
220	1·52	55 28	1·43	52 44
200	1·32	61 14	1·23	56 57

MEAN HEAD and VELOCITY from the EXPERIMENTS.

Quantity of Water Run from Tank.	Temperature 56½°.			Temperature 130°.		
	Time.	Mean Head.	Mean Velocity.	Time.	Mean Head.	Mean Velocity.
Cubic Feet.	Seconds.	Feet.	Feet.	Seconds.	Feet.	Feet.
500 to 400	990·0	3·783	8·42	923·0	3·480	9·03
460 „ 360	1052·0	3·388	7·92	979·0	3·124	8·51
400 „ 300	1169·5	2·798	7·13	1094·0	2·585	7·62
360 „ 260	1276·0	2·399	6·53	1187·0	2·221	7·02
300 „ 200	1514·0	1·789	5·50	1399·0	1·662	5·95

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Corrections for the Effect of Temperature on the Capacity of the Measuring Tank and on the Orifices and Pipe.

Only an approximate value for correction can be determined, as the cast-iron tank and the gun-metal orifices and pipe were exposed to the air on one side and the hot water on the other, and therefore an uncertainty exists as to their mean temperature during any of the experiments. The tank and orifices were measured off and marked at 60°, so that correcting on the assumption that 100° was the greatest rise in temperature during the experiments, the correction must give a far greater variation than could have existed in practice.

The Tables in the Paper are from observed data, as the small amount of correction due to 100° increase does not affect the results, as can be seen from the following calculations:—

The linear extension of brass per unit length and per 100° = 0·001.[1]
" " " cast-iron " " " = 0·00062

The only necessary corrections to be made are for the cubical dilatation of the tank, the superficial expansion of the orifice and the linear expansion of the pipe; the head was in every case measured from a paper strip fastened on to wood, so it requires no correction.

Let Q = observed cubical discharge, Q′ = corrected discharge for 100° rise;
d = observed diameter of orifice, d' = corrected diameter for 100° rise;
L = observed length of pipe, L′ = corrected length for 100° rise.

The formula for the discharge through the sharp-edged or conoidal orifice is $C = \frac{\text{Q per sec.}}{\text{area of orifice}\sqrt{2gh}}$, and if C′ is the corrected coefficient,

$$C' = C \times \frac{(1\cdot00062)^3}{(1\cdot001)^3} = C \times 0\cdot99987,$$

or practically no correction is necessary.

For the pipe experiments:—
Let m' = corrected constant, then as

$$m = \frac{h}{L} \times \frac{d^{3-n}}{V^n}, \text{ and } n = 1\cdot795,$$

$$m' = m \times \frac{(1\cdot001)^{1\cdot205}}{(1\cdot001)\left(\frac{1\cdot00062^2}{1\cdot001^2}\right)1\cdot795},$$

$$= m \times \frac{(1\cdot001)^{3\cdot795}}{(1\cdot00062)^{5\cdot385}} = m \times 1\cdot00046,$$

therefore the corrections may be neglected.

[1] From Clarke's "Constants of Nature."

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(*Paper No. 2172.*)

"On the Wear of Steel Rails."

By HARRY FOOTNER, M. Inst. C.E.

ON the London and North-Western Railway in 1877 iron rails taken out of the line in relaying were credited to the district at 74 lbs. per lineal yard, an average loss of 10 lbs. on the original weight. At the present time 84-lb. steel rails are credited at 66 lbs. The stronger and purer metal retains its form, and the rail generally remains uniform on the head from end to end, until it is considered too light for a safe or easy running road.

The word "generally" is used because steel rails are found which split, crush and laminate.

No information that has been collected and published on the wear of rails gives results sufficiently consistent to guide the permanent-way engineer in determining when a rail should or should not be removed from the road.

A complete investigation of the "Loss in Steel Rails" appears to demand an inquiry under at least the following heads:—

1. Chemical composition of the material.
2. Physical properties of the material.
3. Form of the section.
4. Dimensions of the section.
5. Difficulties attending the process of manufacture.
6. Method of supporting and securing the rail on the sleeper.
7. Materials used for ballast.
8. Curve and gradient, or line and level of the situation.
9. Comparative humidity, &c., of the climate.
10. Speed and amount of traffic.
11. Form and gauge and weight on wheels and wheel-base of rolling-stock.

The Author thought that some more careful observations than had hitherto been recorded, of the changes in the weight and form of rails while in the road, would tend to throw light upon the question, "Why some steel rails, though made at the same time and by the same maker, lasted on one line so much better than on another?" He accordingly selected certain rails, and for several years carefully noted in them the changes of configuration and weight.

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Where so many variable elements are concerned absolute consistency in individual instances was not to be expected; but the results obtained from a large number of cases point unmistakably to the following general conclusions, which will be best exemplified by a few typical sections of rails made of Crewe steel under conditions as nearly alike, as could be obtained on portions of the London and North-Western Railway under the Author's charge. These sections are not extreme cases; there are many instances recorded of greater and less loss under less and greater traffic.

Section A is taken from an agricultural district.

„ B „ „ the neighbourhood of a manufacturing town.

„ C „ „ a few miles from the Chemical Works of Runcorn, Widnes, and Ditton.

„ D „ „ near the Salt District of Winsford.

Sections E and F „ the midst of Ditton Chemical Works.

The outer line of each section is the rail when new, the inner lines of sections A, B, C, and D are the rails after twelve years of main-line traffic, the shaded portion between them therefore represents the loss.

Sections E and F have been down seven years; the latter is from a cross over road laid at the same place and at the same time as E, but over which very few trains have passed.

On comparing these sections one with another two facts are evident: First, that the loss on the heads varies considerably, and does not vary in proportion to the amount of traffic that has passed over them, nor in proportion to the length of life in the road. The loss on section E is three times as great as on section A, although the former has only had 84 per cent. of the tonnage, and 60 per cent. of the length of life of the latter. Secondly, in each of the sections, except section F, the loss on the head bears a tolerably constant proportion to the loss on the remainder of the rail.

Now the total loss on the head of each rail is due to the abrasion of passing trains and corrosion combined, while the loss on the body of the rail is due to corrosion alone. The reduction of the area of the head of rail A is small, and for the moment, for the purpose of comparison with section E, it may be assumed that it is wholly due to abrasion. Except for the impurity of the atmosphere, there is no reason why the head of section E should lose more than the head of section A; consequently, deducting from E an area equal to the loss on A, the remainder, at least, must be due to corrosion. Further, this amount due to corrosion on the head of E is, it will

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be noticed, but little less than the loss on the body of E; but the area of the head or bright surface in contact with the wheel flange is to the rest of the skin of the rail as 1 : 6 (approximately), therefore *the rate of corrosion on the bright surface must be four or five times as rapid as upon the rest of the rail.* Thus the scale forming on the body of the rail affords a lodgment for dirt, and the two together become a natural coating or protection, frequently shaken off, and as frequently renewed, but retarding oxidation; while on the other hand the passing wheel keeps the running surface clean and in a condition most favourable to oxidation. Less exact but no less positive proof of this fact is found wherever two surfaces of iron or steel in contact are kept bright by friction and exposed to moisture; the relative amount of loss by oxidation depends upon the brightness of the surface, and the absolute amount upon the purity of the atmosphere.

Returning to section A, there appears to be no reason why the running surface and the body of the rail should not corrode in about the same proportion as, but in a less degree than, in the case of section E, and it follows therefore that only a small portion of the loss in section A can be due to abrasion. The same reasoning applies to sections B and C; and generally from the foregoing considerations it appears that the abrasion of sound steel rails by an unbraked-wheel is small compared with the amount of loss by corrosion when the atmosphere is impure. The due recognition of this fact leads to some conclusions of interest and importance not only to the rail-maker, and to the permanent-way engineer, but also to all who use metal unprotected by paint or lubricants—steel tires, for example, of which there are thousands of miles in this country, must lose by corrosion an amount hitherto scarcely suspected.

The Paper is accompanied by a drawing, from which Plate 7 has been prepared.

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OBITUARY.

WILLIAM BAYLEY BRAY was born in London, in 1811, where his father was in business as a wine-merchant. He received the greater portion of his education in Germany, and was articled to engineering as a pupil of Messrs. Walker and Burges, being contemporary with a long list of members of the Institution who issued from their office and have attained eminence.[1] Bray gained most of his practical experience on the works of the Leeds and Selby Railway; but his sojourn in the north was cut short by a severe accident, resulting from a fall from some scaffolding, which affected his spine, and made him appear for some years prematurely aged and decrepit. The idleness enforced by a long convalescence, acting upon guileless simplicity of character and deep religious convictions, conspired to render him somewhat ascetic in manner and habits; and imagining all whom he met to be as true and as pure as himself, he frequently became the butt of his more healthful and fortunate companions. On recovering sufficiently to resume his duties, he returned to London, and was engaged principally on works in or near the metropolis; among these, on the Trinity Corporation warehouses and wharves, maintenance and repairs of bridges over the Thames, roads and tramways to the West India and East India Docks, Essex sewers and roads, and the erection of the Priory sea-wall at Hastings. He also set out the works of the single-arched skew-bridge of granite at Stratford-le-Bow, which took the place of the celebrated three-arched bridge of Queen Maud, *circa* 1125.[2] Mr. Bray was also engaged upon the surveys of the Northern and Eastern Railway from Spitalfields to Broxbourne. He then took service with Robert Stephenson, by whom he was entrusted with the conduct of important work in Egypt, and subsequently for several years in Italy on the construction of railways. On completing his Italian engagements, he determined to emigrate, and early in

[1] Among them may be named (Sir) J. W. Bazalgette, G. P. Bidder, M. A. Borthwick, W. B. Bray, J. Cooper, G. S. Dalrymple, T. Drane, E. Druce, A. Drysdale, J. Hartley, (Sir) J. Hawkshaw, C. M. Herbert, D. P. Hewitt, W. H. Holland, J. R. M'Clean, J. M'Connochie, Major Newsome, R.E., T. Ormiston, W. Parkes, F. Pollock, J. B. Redman, T. M. Smith, R. Townshend, and J. Scott Tucker.

[2] Minutes of Proceedings Inst. C.E., vol. i. (1839), p. 77.

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1851 sailed for New Zealand in one of a line of vessels owned by Mr. Frederick Young, the present Honorary Secretary of the Royal Colonial Institute. Mr. Young's father, for many years M.P. for Tynemouth, was at that time Chairman of the New Zealand Company, which first started colonization in this great dependency, and it was by him remarked that Bray went out better provided with "plant" than any colonist they had before had experience of, a large store of agricultural implements and a complete corn-mill forming part of his equipment.

In the autumn of 1851 Mr. Bray landed in Canterbury with his family, so that, although not actually one of the "Pilgrims," his connection with that Province dates back to its first beginning. He then considered his professional life at an end, and began as a farmer, taking up 500 acres under the Canterbury Association at Avonhead, one of the most tempting spots around Christchurch. Mr. Bray was often eccentric in his ideas and ways, and the long curiously-designed house—which, it is said, was built in the peculiar style in order to resist any attack by the Maoris—has often roused the curiosity of those who see it for the first time. But though nominally a farmer, and also partner with the late Dr. Barker in a sheep-run, Mr. Bray by no means completely relinquished the profession. His abilities and thoroughness enabled him in more than one instance to render notable service to Canterbury. Mr. Moorhouse's brilliant idea of piercing the Port hills with a tunnel called for the exercise of much engineering skill, even from the outset. It fell to the lot of Mr. Bray to ascertain the contour levels of the Heathcote Valley in order to settle at what point the tunnel should enter the hills. This task was carried out with all the energy and care which he was wont to expend on his professional work. Later on, however, a matter of far more vital importance to the future of this provincial district was brought to a conclusion under his direction.

It was towards the end of 1862 that the growing necessity for largely extended harbour and wharfage accommodation at Port Lyttelton made itself acutely felt. It was anticipated that the opening of the tunnel would bring about a vast accession of shipping; and the danger and delay which vessels then suffered in having to discharge and take in cargo in a port so fearfully exposed to heavy north-east and south-west weather, rendered it imperative that provision should be made to remedy the existing state of things. To that intent the "Lyttelton Wharfage Accommodation Commission" was appointed by the Superintendent of the Province early in 1863. Mr. Bray was chosen Chairman, and

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with the late Mr. W. J. W. Hamilton, then Collector of Customs at Lyttelton, worked hard and energetically, holding sittings almost daily for more than a month until the report was published. His own high professional skill, backed by the opinion of his old chief, Robert Stephenson, and combined with the practical knowledge of nautical authorities—captains of the White Star line of wool-ships then trading to the port—had the result of fixing the general plan of the harbour works at Lyttelton as they now exist. The recommendations of the Commission brought about the breakwater at Officers' Point, sheltering the Gladstone pier, and many other great improvements.

Amongst what people once thought was only one of Mr. Bray's "wrong-headed" ideas, was this—that the Waimakariri would some day overflow Christchurch. His knowledge of the levels and "lie" of the country showed him that the big river had come down, and would come down again. For years he preached the doctrine of preservation against floods to people who only laughed at him, and who chaffingly told stories of how he had all the levels carefully marked on his dining-table. But all of a sudden, in 1868, these prophecies were fulfilled, and people discovered with consternation, when the water was a foot deep in the old post office, and a stream from Gloucester Street down Colombo Street met a sheet of water in the Market-place, that Christchurch was always in imminent danger. How that question has been set at rest once and for ever people who pay their taxes and rates regularly probably know; but in all probability they do not know who found out all about the danger first. Nor do they know much of the man's inner life now that they know his name, for he lived at all times much to himself, as men of strong religious convictions often do live, and latterly was but rarely seen in the streets of Christchurch. With the exception of one or two visits to England, Mr. Bray had made New Zealand his home ever since his first arrival. He was a man of singular integrity and high principle, and conscientious in the discharge of the duties that fell to him.

Mr. Bray was one of the oldest members of the Institution, having been elected an Associate on the 2nd of February, 1836. He was transferred to the class of Member on the 18th of February, 1845. He contributed two Papers to the Minutes of Proceedings: one "On the Strength of Iron Girders," vol. i. (1837), p. 29; and a "Description of the Ouse Bridge on the Hull and Selby Railway," vol. iv. p. 86. He died at Christchurch on the 26th of May, 1885, in his seventy-fourth year.

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ROBERT DAVISON was born at Belford, Northumberland, on the 10th of May, 1804, and when five years old he with his parents came to London, where he received his education, and in 1818 was apprenticed to Mr. Moorman, of Old Street, then a well-known ironfounder and millwright. In time Mr. Davison became manager to Mr. Moorman, and remained in that position until 1831, when he was appointed resident engineer at Messrs. Truman, Hanbury, Buxton, and Co.'s brewery, which he to a great extent remodelled, and where many of his improvements in the mechanical arrangements are still in operation.

In 1845 Mr. Davison left the brewery and started in practice as a civil engineer, acting also as engineer to the Patent Desiccating Company, which was formed to work his patent process for drying wood and other vegetable and animal substances by currents of hot air,—a process which was largely adopted and is still in use, and for the invention of which Mr. Davison received, from the hands of the Prince Consort, the gold medal of the Society of Arts. Shortly after this he remodelled several breweries, and new ones were erected from his designs; among others one of large size for Messrs. Findlater, of Dublin, his design being chosen out of a number sent in competition in 1852. A few years later he designed and arranged the new brewery for Messrs. Allsopp, of Burton-on-Trent, in which work he was associated with Sir Henry A. Hunt, Assoc. Inst. C.E., this being probably the largest brewery ever erected complete from a pre-arranged plan. In addition to the brewery-work which he had in hand, he in 1858 designed and superintended the erection of a new jetty at the British and Foreign Steam Wharf, and some time afterwards he designed and superintended the erection of an iron pier at Kirkwall.

In 1864 Mr. Davison entered into a partnership for six years with Mr. George Scamell, F.R.I.B.A.; and on the expiration of this partnership, Mr. Davison practised singly till 1875, when he took into partnership his youngest son, Mr. Robert Davison, Assoc. M. Inst. C.E., and Mr. John Mackenzie, M. Inst. C.E., who had been associated with him for some twenty years previously. This partnership continued till 1881, when Mr. Davison, jun., accepted an appointment abroad, and the firm was joined by Mr. George Inskipp, A.R.I.B.A.; Mr. Davison, sen., owing to advancing years and failing health, practically retiring from the business. About this time he went to reside at Finchley, where he expired on the 14th of March, 1886.

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As may be gathered from the above, Mr. Davison turned his attention chiefly to the design of breweries, being the first who made a speciality of this branch of engineering. He designed a considerable number of such establishments (in addition to those mentioned), some of them of large size, and superintended the extension and remodelling of many more. From his experience at Messrs. Truman's he was intimately acquainted with every detail of the business, as well as cost of plant and production, and he was in much request for valuing such establishments, and for giving evidence on arbitrations. He also paid much attention to the details of the mechanism of breweries, and introduced many novelties and improvements in the various machines; among them machinery for raising and conveying malt, and raising and lowering casks, and other labour-saving appliances. He invented and patented several ingenious machines for cleansing casks, the principle being to give the cask a double rotatory motion, the effect being to thoroughly scrub its interior surface: these machines are largely in use in this country and on the Continent.

Mr. Davison was extremely honourable, almost punctilious, in dealing with his clients; and though he had strong objections to treating contractors with unfair harshness, he was strict in enforcing the terms of contracts, which he was very careful in arranging, and he carried on his long term of practice with an almost entire absence of litigation between clients and contractors, and with very few serious disputes. He was a man of great firmness and force of character, and was a good manager of men, having the art of maintaining a somewhat severe discipline, and at the same time conciliating his subordinates.

Mr. Davison was an enthusiastic citizen of London, being a member of the Blacksmiths' Company, which he entered as an apprentice in 1818, was admitted to the Livery in 1835, and, after going through the preliminary offices, he served as Prime Warden (equivalent to Master) in 1857–58.

Mr. Davison was elected an Associate of the Institution of Civil Engineers in 1834, and was transferred to the rank of Member in 1840. At one time he took a considerable interest in the affairs of the Institution, contributing Papers on "A Refrigerator or Machine for Cooling Brewers' Wort;"[1] "Remarks on the Ravages of the Worm (*Teredo navalis*) in Timber;"[2] and a "Description of

[1] Minutes of Proceedings Inst. C.E., vol. i. (1841), p. 57.
[2] *Ibid.*, vol. ii. (1842), p. 90.

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the Mode adopted for Sinking a Well at Messrs. Truman, Hanbury, Buxton, and Co.'s Brewery."[1]

Mr. Davison also acted as Auditor of Accounts of the Institution for the years 1849 and 1850.

DAVID SCOTT, the youngest son of Dr. Scott, of Cupar, Fife, was born near Cupar, on the 12th of March, 1838, and died at Melbourne, Australia, on the 29th of August, 1885.

He was educated at the Madras College, St. Andrews, and in 1854 was articled to Messrs. Balfour and Co., of the Ironworks, near Leven, Fifeshire. He served five years with this firm, passing through the usual training in the drawing-office, pattern-shop, fitting-, and erecting-shops, to qualify as a Mechanical Engineer. Having completed his apprenticeship with this firm, he, in 1859, returned to St. Andrews, where he studied under Dr. Sonie, with the object of competing for an appointment in the Public Works Department of India, and in June 1860 passed seventh in the open examination held at Addiscombe. He sailed from England in August 1860, and on his arrival in India he was sent to the Civil Engineering College at Roorkee, where he remained studying Oordoo, and the technicalities of Indian engineering, till July 1861, when he was appointed a second grade Assistant Engineer on the Permanent Staff of the Public Works Department, and posted to Oudh. In Oudh he was employed on the construction of the Trunk Road between Lucknow and Fyzabad, under Captain Pemberton, R.E., and carried out the section of that road between Burrabunkee and Byramghat, and the large bridge over the Kallianee river. In 1866 he was promoted to the fourth grade Executive Engineer, and posted to the charge of the station and Cantonments of Seetapore, where he remained till 1870, when he was transferred to the Irrigation Department, and appointed an Executive Engineer on the staff of Major Forbes, R.E., who had been deputed to get up the project for the Sirhind Canal. On the surveys and plans for this great work, Mr. Scott was employed till September 1871, when the scheme was abandoned under orders from the Secretary of State. The stoppage of this canal was a great disappointment

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[1] Minutes of Proceedings Inst. C.E., vol. ii. (1842), p. 192; vol. iii. (1843), p. 57.

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to Mr. Scott, who had looked forward to carrying out a portion of the works he had assisted in designing. But the good service he had rendered, and the ability he had shown, were fully appreciated, and on the staff being broken up, Mr. Scott was appointed Assistant Secretary to the Government of India in the Buildings and Road Branch, and acted in that capacity till he took furlough to Europe in March 1873. On his return from furlough he was appointed to officiate as Vice-Chairman and Engineer to the Port Trust Commissioners of Calcutta. On vacating that appointment, the Commissioners placed on record their appreciation of the services he had rendered in the following resolution:—

> "Resolved.—That a vote of thanks to Mr. Scott be recorded for the manner in which he has conducted the duties of the Vice-Chairman during the absence of Mr. W. D. Bruce, on leave."

Mr. Scott then rejoined the Public Works Department, and continued to fulfil the duties of an Executive Engineer in that department, in various parts of Bengal, till May 1881. During the famine in the Madras Presidency, in 1877, he was deputed to take charge of famine relief-works, and rendered excellent service, under very trying circumstances, in the Bellary and Salem districts of that presidency. In May 1881 he was again appointed to officiate as Engineer to the Port Commissioners, Calcutta, which office he held till May 1883, when he was again obliged to proceed on furlough to recruit his health. On his return from furlough, the project for wet-docks at Kidderpore, which had been submitted by the Port Commissioners and sanctioned by the Secretary of State for India, was commenced, and he was then appointed Superintending Engineer of those works. In June 1885 an attack of liver complaint compelled him to proceed on a sea voyage to Australia. He landed in Melbourne on the 12th of July, very little improved by the voyage, and gradually sank till he died there on the 29th of August, another victim to that tropical disease which causes so many gaps in the ranks of the engineers in Bengal. His death was deeply regretted by a large circle of friends, to whom he had, by his gentle manners and kindly disposition, endeared himself.

Mr. Scott was a great reader, and loved his books, and few engineers had a more extensive knowledge of all that was going on in the engineering profession throughout the world. He was held in great respect by all who knew him, both for his professional attainments and personal character, and his conduct was

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invariably marked by the highest honour and integrity. His life was spent in an earnest desire to do his duty, both in his public and private capacity, and those who knew him best know how well he succeeded.

He was elected a Member on the 1st of April, 1879.

THOMAS MACDOUGALL SMITH (affectionately styled "Tom Smith" in the closer circle of his many friends) was for nearly half a century one of the best known and most generally popular of the older members of the Institution; a man whose genial temperament and courteous manner possessed a magic power of allaying the friction engendered in the course of modern professional life, and who, in the course of a long, well-filled career, attained a reputation which, if less commanding, was perhaps more lasting than that of many who have surpassed him in engineering achievements.

He was the son of Thomas Smith of Newcastle-on-Tyne, where he was born on the 1st of November, 1826. About the year 1835 Macdougall Smith entered the office of Mr. Walker, having previously served a pupilage to Messrs. Hawks and Co. of Newcastle-on-Tyne (where he was considered somewhat of a deft hand in finish at the bench and the lathe), and then on the Leeds and Selby Railway. The office of Walker and Burges was at that time the great nursery of civil engineers in England, that is to say, of civil engineers of the Smeatonian school, who, so far, were the only recognized exponents of the art; the brilliant achievements of the Stephensons and Brunel not having yet won for railway-making its subsequent ascendency in engineering. In this office Macdougall Smith was contemporary with a numerous band of pupils who one and all attained good positions in after-life.[1] He speedily developed into a first-class draughtsman, and an excellent and careful hand in the field. He had a passion for finishing up drawings, and several amusing anecdotes are told of his (sometimes misapplied) skill in this direction. At this time he was engaged under Mr. Burges for the Essex Sewers Commissioners in putting in new sluices through the river-bank behind cofferdams at Hornchurch and Dagenham, and made a survey of the site of the historic breach and remains of Captain Perry's successful works. The infinite pains taken by him in all the details of those appa-

[1] *Ante*, p. 439.

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rently trifling, but really important operations, the whole levels being exposed to any casualty occurring from carelessness or inadvertence, was characteristic of the man's sterling qualities and trustworthiness. His purity of character and gentleness of manner endeared him to all with whom he was long in contact. His colleague, Mr. J. R. McClean (afterwards President of the Institution, and M.P. for South Staffordshire), was greatly attached to him, and these men, apparently so dissimilar, were in early days found much together, roughing it gaily on surveying-expeditions, and making good stories of their adventures with navvies and natives.

His Paper on the celebrated "Pont-y-tu-Prydd,"[1] with copy of the well-known engraving of old Edwardes, was a sample of the way he minutely finished his work. Mr. Alfred Burges, formerly M. Inst. C.E., had a singular tenacity for locality or a devotion to what might be termed the "Home Circuit," and bestowed upon the small details of such work as Trinity lighthouse additions, Metropolitan roads and bridges, sewers and river-docks, an amount of time and attention which appeared to some, connected with the more ambitious work of the firm, rather thrown away, and he was an anomaly in that he became a leading marine engineer of his day without ever having crossed the seas or visited the Channel Islands, where his firm carried out such extensive works. In Macdougall Smith, Burges found a willing and painstaking co-adjutor. On leaving Messrs. Walker and Burges, he was appointed to the engineering staff of the London and Birmingham Railway, and became Resident Engineer of the Leamington branch line in 1844. Subsequently, for Mr. McClean, he was entrusted with the surveys for the South Staffordshire line, and later, the levels for the Henley water project were taken by him, also for Mr. McClean. He afterwards became concerned in mining-work, to which he was mainly devoted during the remainder of his career, although he occasionally made excursions into the domain of his early surveying experience, as, for instance, when he laid down the line and levels for the keel of the "Great Eastern" steamship, in which piece of delicate work he acquitted himself to Mr. Brunel's satisfaction. In conjunction with Mr. T. Sopwith he wrote a report on the Belgian coalfields,[2] and of late years became iden-

[1] Minutes of Proceedings Inst. C.E. vol. i. (1838) p. 36.

[2] Reports on the Ironworks, Manufacturing-Establishments and Mineral property at Couvin in the district of the Sambre and Meuse in Belgium. By Thomas Sopwith and T. Macdougall Smith. 8vo. Map and Plan. London, 1846. Inst. C.E. Tracts 8vo. Vol. 384.

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tified with mining- and quarrying-operations in Wales. An instance of his disinterestedness may be mentioned as an indication of the man's character. A kinsman of one of his friends was bitten by the taking appearance of a coalfield in Wales, and by the would-be vendor's picture of its advantages. Smith at once showed him that it was on the edge of the seam, and of very doubtful value. This opinion, entirely confirmed by the highest authority of the day at the Royal School of Mines, was given without fee or reward, and the same careful attention bestowed on the matter as if encouraged by a heavy retainer.

His interest in the Institution was displayed periodically by his undertaking, for more than twenty years, the office of one of the scrutineers of the ballot for the election of the President and Council, a piece of tedious drudgery which can only be confided to members of the Corporation taking part in the Annual Meeting. In order to lighten this labour, which is carried on at high pressure for an hour and a half, and the results of which are impatiently expected in the meeting-room, Mr. Smith borrowed, from the coal-scores in collieries, a system of grouping the votes against each candidate's name, in such a way that instead of finding, when counting-time came, a confused mass of units, each to be counted separately, there was presented a small and easily-separated number of 𝍸 𝍸. In this way that which might otherwise be likened to a line of letters became a line of words, and the labour of counting up the votes was greatly lessened.

His somewhat sudden collapse in health was a surprise to many of his friends, for although attaining the allotted human span of three score years and ten, his well-proportioned frame and the thorough working order of the living machine, resultant on a well-ordered and temperate life, enlightened by a clear conscience, would lead many to suppose he was a decade younger than in reality he was, and that many working years were yet in store for him.

An intimate acquaintance of fully half a century leads the compiler of this notice to surmise that the terms used above, apparently eulogistic, are in reality in nowise exaggerated.

Macdougall Smith's life illustrated the truth—

> "Better is he that ruleth his spirit (and he had one)
> Than he that taketh a city."

He was elected a Graduate on the 24th of March, 1840, having previously received a Telford Medal for his Paper on the Pont-y-tu-Prydd. Six years afterwards, on the 17th of February, 1846, he was transferred to the class of Member. He died, to the great

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grief of his family and friends, and to the unaffected regret of a very large circle of acquaintance, on the 21st of January, 1886. Four days later his remains were interred in Brompton Cemetery, in the presence of a representative concourse of the friends and associates among whom, in the very headquarters of engineering, he had passed the greater part of his life.

ALEXANDER ANDERSON KYD, son of Mr. David Kyd, jute manufacturer, Rosebank, Dundee, the youngest of a family of five sons and three daughters, was born on the 18th of November, 1856. He was educated at the High School of Dundee, and at the University of Edinburgh, sessions 1876–1879, where he gained four medals, and obtained first-class certificates in each of the eight classes which he attended. The late Mr. Fleeming Jenkin, F.R.S., M. Inst. C.E., Professor of Engineering in that University, testified not only to his marked ability, but to the striking practical knowledge which he showed of engineering subjects, especially those bearing on water-supply and sanitation.

He served an apprenticeship of four years in the office of Mr. William Mackison, F.R.I.B.A., surveyor and engineer for the burgh of Dundee, and continued for a short time as Mr. Mackison's assistant after the close of that period. Works of great extent were then being executed by the municipality of Dundee, £140,000 having been expended in town-improvements, streets, tramways, and bridges, drainage-works, markets, and swimming-baths.

In 1878–1879 he assisted Mr. R. Ellis, Edinburgh, resident engineer of the Uddingston and Holytown branches of the Caledonian Railway. He was occupied also in field work on the Callander and Oban Railway, of which, during construction, Mr. Ellis was consulting engineer. In 1879–1880, under engagement with Mr. G. A. Lundie, M. Inst. C.E., he made a trigonometrical survey of the burgh of Galashiels for sewerage purposes. On the completion of that work he entered the service of Mr. Alexander M'Culloch, Assoc. M. Inst. C.E., assisted him in connection with the laying out of the Duthie Park and the Trinity Cemetery at Aberdeen, and took a leading part in the construction of Carnoustie Waterworks for the local authorities of Barry and Panbride in the county of Forfar. In 1881 Mr. G. A. Lundie entered into another engagement with him, under which he superintended the construction of the Kenilworth Sewerage and Sewage-Farm Works. After the completion of the main

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drainage scheme he was appointed surveyor to the Kenilworth Local Board. He prepared for deposit with the Board of Trade the plans and sections of the Kenilworth Waterworks, and continued his services to the local authorities there till the end of 1883.

Early in 1884 he was appointed one of the engineering staff of the London Sanitary Protection Association, a post which he held until his death. As engineer of that Association he was employed to report upon the drainage and sanitary arrangements of many large and important buildings in London and the neighbourhood, such as Eton College and all the Masters' houses there; the Royal Indian Engineering College, Cooper's Hill; Bexhill Convalescent Home, and many others, as well as large numbers of private houses; and in most of the cases he had to supervise the execution of the alterations which he recommended. All his work gave the greatest satisfaction to his immediate superior, the consulting engineer, and to the Council, who expressed their sense of the great loss they had sustained by his death in a minute which was communicated to his widow and in private letters addressed to her by the Secretary of the Association.

On the 19th of January, 1886, being to all appearance in his usual health, and having attended to his professional work for the day, he went in the evening to a mission in connection with Clapham Congregational Church. Shortly after his return home he was seized with blood-spitting, which in the course of the night developed into vomiting of blood, continuing at intervals for two days. Acute phthisis supervened, cutting him off in the space of three weeks. He died on the 11th of February following.

Mr. Kyd was elected an Associate Member on the 5th of December, 1882, having previously been a Student. He was a man of literary taste and ability, and now and then engaged in public speaking and writing. He described the International Health Exhibition in a series of letters to the *Dundee Advertiser*; his other appearances in print being of a like ephemeral character. He devoted his leisure time chiefly to church and mission work, taking an especial interest in young people. He was secretary of the Clapham Congregational Young Men's Union. As a vocal musician he possessed good abilities, which he sometimes employed in choir-training.

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ALBERT HARRISON TURNER, second son of the late Mr. Albert Turner, a London solicitor, was born on the 4th of March, 1857. He was educated at Sevenoaks Grammar School, and subsequently spent two years at the Crystal Palace Company's School of Engineering, under Mr. J. W. Wilson, M. Inst. C.E. He left there in 1875 to take up a position under Messrs. Christopher James and Co., colliery proprietors, with whom he gained considerable experience in mining. His first responsible charge came in 1880, when he was chosen by Messrs. F. and A. Swanzy, to undertake the working of a gold mine they had purchased at Wassau, on the West Coast of Africa. Here he superintended the erection of a quantity of mining-machinery, stamps, pumps, saw-mills, &c., with very little European assistance, the work being principally done by natives, of whom some hundreds were under his orders, and in managing whom he showed considerable tact. In a letter to his old chief at the Crystal Palace Engineering School, he gave an interesting account of the mode of working the mines, which had been taken over by a limited company under the style of the Wassau Gold-mining Company, and of life in the forest or bush. He at first seemed to scout the idea of the Coast being unhealthy, and maintained that the ill-repute of the place arose from the death of those men who came out from England and indulged in all kinds of excesses, which would be dangerous even at home. But after three years' residence at Wassau he deemed it advisable to take a six-months' holiday to England. Two years later he had an attack of dysentery, and again came home. He returned to the Coast in April 1885, but eight months afterwards was struck down by jaundice, complicated with severe fever, from which he died after a few days' illness, on the 5th of December, 1885.

Mr. Turner was a young man of great promise, as was indicated from his obtaining such a responsible position so early in life. He was elected an Associate Member on the 5th of February, 1884.

JOHN ROBSON WARHAM was a native of Newcastle-on-Tyne, where he was educated, and subsequently served an apprenticeship to Messrs. R. Hawthorn and Co. When about twenty-two years old he went to Burton-on-Trent as manager for the late Mr. R. Thornewill, ironfounder. In 1849 he became a partner, and since then had continued a member of the firm of Thornewill and Warham. His activity, industry, and ability did much to extend the operations of the firm, and to give them a name

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which is known all over the kingdom—associated, as he was, in connection with numerous important collieries, breweries and waterworks. While, however, thus helping to further establish and extend his firm, he did not neglect the local public interests. He was one of the oldest Feoffees; was for many years a Town Commissioner, and, while in that position, was one of the most active and prominent members of the Gas Committee; was a member of the Board of Guardians for Burton for a considerable number of years, and was the first elective member of the Board to become Chairman, which position he held for about three years; was elected a member of the first School Board, and served on that body for the first triennial period; and was twice churchwarden of the parish church—the first occasion being from 1867 to 1872, and again later on. In his more social engagements he made many friends, his genial and generous nature earning for him the esteem of all with whom he came into contact. His private liberality was very great, but of the most unostentatious character, and his decease has deprived the poor of the town of a true friend and helper.

Mr. Warham was elected an Associate Member on the 6th of May, 1862, and was also a Member of the Institution of Mechanical Engineers. He died from bronchitis on the 26th of March, 1885, in his sixty-fifth year.

*** The following deaths occurred in the three months ending on the 31st of March, 1886, in addition to those included in the foregoing notices :—

Members.

AVERN, FREDERICK MORRIS, on the 15th of March.
LEECE, JOSEPH, on the 12th of January.
NEWCOMBE, EDWARD, on the 17th of January.
RICHARDSON, JOSHUA, on the 22nd of March.

Associate Member.

PONTIFEX, SAMUEL, on the 25th of March.

Associates.

MONTGOMERIE, *Lt.-Col.* PATRICK, R.E., on the 8th of January.
OGILVIE, ALEXANDER, on the 15th of February.

Information respecting the careers and leading characteristics of any of the above is solicited, to aid in the preparation of future Obituary Notices.—SEC. INST. C.E.

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Sect. III.

ABSTRACTS OF PAPERS IN FOREIGN TRANSACTIONS AND PERIODICALS.

The Squadro-Cyclograph.

(L'Ingegneria Civile, 1885, p. 81.)

This is an instrument (invented by Messrs. Pesso and Perilli, who were engaged in setting out a line of railway from Lecco to Como) for setting out railway curves, for which purpose it was found to give accurate results in much less time than the ordinary methods. It is particularly useful for ascertaining, by trial on the ground, what curves are best adapted for working round awkward places. The instrument is in appearance similar to an ordinary cylindrical optical square, but, in addition to the angles of 90° and 45°, it has also sights of 1°, 2°, 3°, 4°, 5° and 6° to right and left of zero. In setting out a curve of any radius, it is necessary to calculate the length of the chord for a tangential angle of 1°, which is readily done by multiplying the radius by twice the sine of half the arc, or $R \times 0{\cdot}034904$, which number, for convenient reference, is engraved on the instrument. Supposing, therefore, that it is required to set out a curve of 1,000 feet radius, the length of chord in this case is 34·904 feet; this length is then marked out on a tape or chain, and given to a couple of assistants, and the curve is then set out in a manner similar to that adopted with the theodolite. Only six chords can be set out from one position of the instrument, which must then be moved to a new station, from which the work can be continued. The method of solving various problems which occur in practice is given in the Paper, and also answers to objections which have been made.

The Engineering Society of Turin appointed a committee of engineers to examine and report upon the instrument, and their conclusion was that the degree of accuracy attainable was sufficient for all ordinary purposes, and that, particularly in mountainous districts, the system was better than any of those in ordinary use.

W. H. T.

Researches on the Constancy of Volume of Cements. By Dr. Böhme.

(Mittheilungen aus den königl. technischen Versuchsanstalten zu Berlin, 1885, p. 94.)

For determining the constancy of volume the standard Prussian test is this: Pure cement is mixed with water to a stiff cream, and formed into a thin cake on glass, or metal plates, or on a roof-

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tile saturated with water. After setting, cake and plate are placed under water. If the cake shows after one or more days crumpling, or cracks at the edge, the flying (*treiben*) of the cement is indicated.

Another test not prescribed is the so-called baking-proof. The cement, mixed with water to a syrupy consistence, is poured on a gypsum plate covered with damp filter paper. In about ten minutes the cake is placed on a heated iron plate, and baked for an hour. If the cake remains sharp-edged and free from cracks, the cement is constant in volume.

A third test proceeds on the hypothesis that the quick hardening of cement in boiling water proceeds just as in water of mean temperature. The cakes formed on glass plates are hardened twenty-three hours in air, and then placed in boiling water. The least tendency to flying of the cement is shown in ten to sixty minutes.

The thirty-nine cements used in the above research were tried for constancy of volume in each of these three ways. One hundred and eight other cements were also tested.

1. Of the 39 cements,
   26 stood the standard and baking and cooking test;
   35 the standard and baking proof;
   38 the standard proof.
2. Of the 108 cements,
   89 cements stood all three tests;
   106 the standard and baking test;
   108 the standard test.

W. C. U.

On the Influence of the addition of various Pulverized Substances to Portland Cement. By Dr. Böhme.

(Mittheilungen aus den königl. technischen Versuchsanstalten zu Berlin, 1885, p. 78.)

Three series of researches were undertaken. A. Researches with a pure cement, and its mortar formed with two kinds of sand mixed with seven different substances. B. Researches with cement mortars of thirty-nine different cements, with and without an addition of slag. C. Researches with cement mortars with an addition of slag and gypsum.

A. A Portland cement of average quality having been selected, it was tested for tension and compression, as pure cement, as mortar of one part cement, three parts standard sand, and as mortar of one part pure cement, three parts Berlin sand. Tests were made at seven, twenty-eight, ninety, and three hundred and sixty-five days. Corresponding tests were made with neat cement and cement-mortar briquettes in which 10, 20, or 50 per cent. of the cement was replaced by the following substances: (1) finest cement; (2)

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finest sand; (3) Duisburg slag; (4) Upper Silesian slag; (5) trass; (6) brick powder; (7) limestone powder.

The addition of (1) diminished the tensile strength of pure cement, but increased that of the cement mortars. For all the other additions the tenacity both of cement and cement-mortars is diminished.

As to the compression tests, the cubes of pure cement and pure cement with the additions give results similar to those for tension. But the strength of the cubes of cement-mortar with the additions is sometimes equal and sometimes greater than that of those without the additions.

B. To try if the same conclusion would hold good with other cements, thirty-nine German cements were selected and tested as pure standard cement-mortar without additional material, and also with an addition of 20 per cent. of Duisburg slag. Of these, twenty-nine cements showed the greater tensile strength in all stages of hardening when without any additional material. The cements with 20 per cent. addition of slag lost nearly that proportion of strength.

C. Two cements were tried in the following ways: (α) 100 parts cement, 300 parts standard sand; (β) 90 parts cement, 10 parts slag, 300 parts sand; (γ) 90 parts cement, 8 parts slag, 2 parts gypsum, 300 parts sand. In all stages of hardening β was weaker than α; but at twenty-eight days γ was about as strong as α, and at ninety days it was stronger. The increased strength, however, is known to be obtained at the cost of trustworthiness.

W. C. U.

Porte de France Cements. By C. TALANSIER.

(Le Génie Civil, vol. vii., 1885, p. 369, 19 woodcuts.)

The material for the Porte de France cements is quarried from the top of Mount Jalla, which rises above the town of Grenoble. It consists of clayey bituminous limestone, and the principal seam has a dip of 75°, and a thickness of 15 feet. The main heading is 1,300 feet above the mouths of the kilns. The rock from the nineteen other headings is delivered into the main heading, from which it is conveyed in trucks, holding 2½ cubic yards, on a line along the side of the hill for a distance of half a mile, where reaching the edge of a precipice, 1,000 feet deep, it is carried in skips down the steep mountain-slopes, hanging on a suspended cable, 1,970 feet long, along which the skips travel. The actual vertical distance is 1,017 feet; and the ton load of stone is borne by a steel-wire cable, 1¾ inch in diameter, which is anchored in the rock at the top, and is kept stretched by being wound round a drum at the bottom. A second cable carries another skip, which is connected to the first skip by an endless cable, so that the loaded skip in descending draws up an empty skip to be filled. This connecting

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endless cable, 0·7 inch in diameter, goes round a brake-pulley at the summit, and round a second pulley at the base, which is fastened on a loaded frame running on four wheels up or down an inclined plane, so that the tension of the cable is kept constant, and the motion of the skips is regulated.[1] The ascent of a skip occupies about one and a half minute; and the whole operation, including loading and emptying, takes three minutes. These cables, erected in 1874 at a cost of £620, admit of a supply of 120 to 150 tons of stone per day of twelve hours. The results were so satisfactory, that a second set of cables were erected in 1875; and a lower quarry is similarly supplied, but the cables are only 1,000 feet long. The kilns, forty-six in number, are situated at the foot of Mount Jalla, having an average capacity of 80 cubic yards. One ton of stone is burnt with 550 lbs. of anthracite to produce the slow-setting cement, and with 400 lbs. of anthracite-dust to form the quick-setting cement. The former, termed natural Portland cement, is black and very heavy, and the latter is yellowish and much lighter; they can be readily sorted by hand, and they are then ground. Four qualities of cement are manufactured, namely, quick-setting cement; natural Portland cement, rather slow-setting; slow-setting artificial Portland cement, and white Portland cement. The chemical constituents of the three first, and of English Portland cement, are as follows:—

	Quick-setting Cement.	Natural Cement.	Artificial Cement.	English Cement.
Silica	22·10	22·61	25·20	24·10
Alumina	18·21	19·79	11·40	10·30
Oxide of iron	traces	..		
Lime	55·98	51·63	56·10	61·40
Magnesia	0·37	0·37	2·40	0·50
Sulphate of lime	3·34	5·60	..	..
Sulphuric acid, alkali water, &c.	..	..	4·90	3·70
Total	100·00	100·00	100·00	100·00

The quick-setting cement sets in five minutes, when used neat, and soon acquires a considerable tenacity. It has been employed in cement-concrete to form conduits for water under pressure and for telegraphic transmission, in the construction of drains and large culverts, and for the facing and strengthening of tunnels. The natural cement sets in thirty minutes, and soon acquires a tensile strength superior to the quick-setting cement. The rapid setting of the latter in hot weather can be checked by an admixture of natural cement. A mixture of the two cements facilitates greatly the moulding of artificial stone to serve as ashlar. The natural cement is very generally used for coating reservoirs, which could

[1] Minutes of Proceedings Inst. C.E., vol. xlix. p. 351.

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not be so neatly effected over large surfaces with rapid-setting cement. The artificial cement, used neat, sets in three hours; it is largely employed for forming foot-paths and slabs. Concrete formed with this cement has given satisfactory results in roadways at Grenoble; and where its slow-setting would cause too long an interruption of the traffic, it has been laid down in the form of slabs, which have previously been given time to set. It has also been adopted for concrete walls, bridges, and blocks. The white cement is used for making pressed tiles, and for pointing white masonry and marble.

L. V. H.

Fall of a Concrete Arch on the Athens and Piræus Railway.

By P. SCHNEIDER.

(Schweizerische Bauzeitung, 1885, p. 109.)

For various reasons cement was much used in the construction of the Piræus-Peloponnesus railway for concrete pipes from 6 inches to 3 feet 3 inches in diameter, and arches of from 2 to 98 feet span, also for abutments and piers of iron bridges. Hitherto (1885) more than seventy arches, varying in span from 6 to 33 feet, have been constructed, including a viaduct of four openings of 31 feet span. With the exception of the 98 feet span arch which fell, the experience with these concrete works has been very favourable. The radius of the intrados of this arch was 88·6 feet, and of the intrados 72·2 feet at the crown and 62·3 feet at the springing. The thickness at the crown was about 3·9 feet, and at the springing 6·1 feet. The width at the crown was 13·1 feet, and at the springing 15·1 feet. Instead of the spandril walls being carried up solid, the roadway was carried upon spandril arches, two on each side of the main arch. The dimensions of the arch were so arranged that the line of pressure for the most unfavourable loading should fall within the inner third of the depth of the arch, and that the maximum edge-pressure should not exceed 227 lbs. per square inch.

The concrete was mixed in the following proportions:—

One of Portland cement [Desiré Michel].
Two of sea-sand.
Four of stone, broken to pass a 2-inch ring.

Sea-water was used, as it had been found to answer well in the smaller works, and fresh water would have had to be brought from a distance of 5 miles.

The broken stone was washed before being used, and the concrete was punned in.

The other works on the line had been made with concrete in the proportions 1 : 3 : 6, instead of 1 : 2 : 4.

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The scaffolding and centering were about 70 feet high to the crown of the arch, but for a short distance from each abutment the laggings were supported by short struts direct from the surface of the ground. When the concreting had proceeded to about 13 feet up from the springing, a vertical crack appeared in the arch over the face of each abutment, owing to defective carpentry in connection with the short struts above referred to. After the centering had been readjusted, these cracks were found to be about ½-inch wide. They were carefully filled with cement, and additional thickness was given to the abutment, and also a small amount of solid backing was added to this portion of the arch. With this exception, the arch was successfully completed and the spandrils filled in. Sixty-nine days after the completion of the arch the centres were slackened by loosening the fox-wedges, commencing with those nearest to the centre of the arch. Before the removal of the last wedge the crown of the arch had sunk 1 millimetre, and on their removal 2 millimetres (0·08 inch), and the abutments had not given at all. Careful examination showed that several small cracks had appeared in the spandril walls, and the following morning cracks were found in several places in the arch. The removal of the scaffolding was, however, proceeded with, as these cracks were not held to threaten the stability of the arch; but while the last row of uprights was being removed, the arch fell.

Examination of the concrete showed that some of it was defective, soft, damp masses being found adjoining hard, well-set portions. The fall of the arch is attributed by the Author to the varying quality of the cement.

W. B. W.

Resistance of Materials in Retaining Walls. By — HÉTIER.

(Annales des Ponts et Chaussées, 6th series, vol. ix., 1885, p. 795, 61 woodcuts.)

Without attempting one of Prof. Boussinesq's learned analyses,[1] or pursuing and discussing Mr. Gobin's experiments,[2] the Author has endeavoured, by means of results attained, and with the help of the principles of resistance of materials, to obtain formulas and practical methods for the construction of stable walls of reasonable dimensions. This investigation is the more necessary, as Mr. Gobin assumed that a wall, in yielding to the pressure of earth at the back, turns upon an outer edge of unlimited strength; and starting from such a purely theoretical hypothesis, he necessarily arrived at results which might lead to the construction of much too slender walls. At the outset, the following practical rules should be borne

[1] "Essai théorique sur l'équilibre des massifs pulvérulents," and Minutes of Proceedings Inst. C.E., vol. lxxii., p. 262.

[2] Minutes of Proceedings Inst. C.E., vol. lxxvi., p. 387.

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in mind, namely:—that the work should be executed before slips have commenced; that the wall should have a large base and a small height; that an efficient system of drains should be formed at the back; and that dry stone pitching should be avoided. Both reservoir-dams, exposed to the pressure of water, and ordinary retaining-walls are considered in the article; but the latter walls are limited to the case of earth levelled flat with their top at the back. A well-built retaining wall, with a good foundation, is liable to fail by the perishing of the mortar, which, under the influence of continued rains, and the vibration of heavy carts, is eventually crushed, a crack is formed, and at last a portion of the wall gives way, showing the great importance of repointing the joints of retaining walls. The pressure of the water against a reservoir dam can be determined with mathematical accuracy; but the pressure of earth against a retaining wall is a far more difficult problem. It is necessary, in this latter case, to provide against the greatest pressure which the earth at the back may be liable to exert; and, neglecting therefore the friction against the wall, Coulomb's formula may be adopted in practice. Consequently, the thrust against the vertical back of a wall amounts to $K y^2$, and is a horizontal force acting at two-thirds of the depth y of the filling behind from the top of the wall. The coefficient K is given by the formula—

$$K = \frac{d}{2} t g^2 \left(\frac{\pi}{4} - \frac{\phi}{2}\right), \text{ or (Rankine)} = \frac{d}{2} \frac{(1 - \sin \phi)}{1 + \sin \phi}, \text{ or}$$

$$\text{(Gobin)} = \frac{d}{2} t g^2 \frac{a}{2},$$

where d is the weight of a unit volume of earthwork, and ϕ the angle between the natural slope of the earth and a horizontal line. The pressure, therefore, in this case, is the same as that of an imaginary fluid whose density is proportionate to K. After deducing formulas for the maxima compression and tension under various conditions, it is pointed out that the profile of reservoir dams can be calculated by dividing them into a series of horizontal sections, and that as the outer face is subject to compression only, it can be economically constructed with squared rubble in courses normal to the face. The second chapter is devoted to the consideration of reservoir dams of various forms. In such works it is specially necessary to provide against the tensional strains on the inner face, for the formation of fissures in the joints might endanger the stability of the dam; and, consequently, it is advisable to construct the inner face with random joints, the stones being arranged with their greatest length vertical where the tensional strain is considerable. In the case of very high dams, the tension on the lower part of the inner face may be small with the reservoir full; whilst the compression may be sufficiently great when the reservoir is empty, to render it advantageous to build this portion of the face with horizontal joints, so as to offer more resistance to

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compression. The great superiority of a curved outer face and a vertical inner face for a dam, over the reverse form, or even over both faces equally curved, is demonstrated; and it is shown that the advantage of an overhanging inner face is not sufficient to warrant the difficulties which its construction would entail. The chapter concludes with the following practical rules in construction, namely, that the outer face should be built with squared rubble, in horizontal courses vertical to the face, well bonded, and the joints deeply picked out and repointed in cement-mortar after the completion of the dam, to provide against previous tensional strains; that the main body of the dam should be constructed of rubble work, avoiding horizontal courses; and that the inner face should be formed with random joints down to a depth of 164 feet, but that below this limit courses of squared rubble should be adopted, and that the repointing should be effected as soon as the masonry has been raised about 15 feet above. The third chapter deals with reservoir-dams having counterforts, of which only those with external counterforts are considered, as being the proper form to adopt. Formulas are obtained for these types of dams, and applied to certain cases, illustrating the value of counterforts in reducing the requisite amount of masonry, and showing that they reduce the tensional strains whilst augmenting compression. Provided the facework is roughly built, considerable economy is effected by the use of counterforts, especially where masonry is costly. The bulk of the wall should be built of rubble masonry thoroughly bonded vertically; and the inner face should be formed of random rubble. Ordinary masonry retaining-walls are next considered, by inserting the coefficient K, for the thrust of the earth, in the formulas already obtained for reservoir dams. The conclusions arrived at are: that the inner face should be made vertical whenever practicable; that where the batter of the outer face must be limited, the adequate bottom width may be provided by stepping out the back of the wall, whilst, on the contrary, where the outer batter must be excessive, the inner face may be made to overhang. Owing to the comparatively small pressure on the outer face, it may be built of random masonry well pointed. The inner face, being hidden, may be left rough; but, as it has to resist tension, the masonry must be solidly and carefully built. Small drainage holes must be provided near the base of the wall, communicating with rubble drains, if necessary, at the back. Retaining walls with counterforts are then similarly investigated. Lastly, dry rubble retaining walls and pitched slopes are separately dealt with. Though counterforts are preferable on the outer side of retaining walls both for economy and stability, they sometimes have to be placed on the inside, as for instance in quay walls; but then they must be very firmly connected with the main wall. The judicious use of arches for such walls furnishes the best means of connecting the counterforts. Where stone is cheap and lime is dear, as for instance in mountainous districts, dry masonry retaining walls are economically advantageous. In ordinary cases,

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these dry walls should have a batter of 1 in 5 on the outer face, and a smooth curved face on the inner side without any steps. The wall should be built as compactly as possible, and horizontal joints avoided. The outer face should be laid in slow-setting mortar for a depth of about 21 inches, and after the completion of the wall, the joints should be scraped out and pointed in cement or hydraulic mortar. Wide vertical joints, left open, at the lower part of the wall, at regular intervals of from 6 to 10 feet, should be provided for drainage.

L. V. H.

On the Determination of Dimensions of Iron Structures.

By Prof. R. KROHN.

(Zeitschrift des Vereines deutscher Ingenieure, 1885, p. 153.)

In calculating the dimensions of an iron structure it is necessary, (1) to ascertain the strains in the various parts of the structure, and (2), on these to base the cross-sections of the various parts. The methods of doing (1) are now thoroughly understood, but (2) has only of late years received attention. The more it is studied the more difficulties it presents, and up to the present time it has not been satisfactorily solved. Until lately a certain specific stress was assumed for each material as permissible; for instance, for wrought-iron 3·8 to 5·1 tons per square inch, giving a factor of safety of from 4 to 6, without any definite reason being given for exactly this proportion, being assumed. The experience of structures which had done well, dimensioned in accordance with these figures, was taken as enough, but such experience could only prove that it was not necessary to introduce a greater co-efficient of security; it gave no indication as to whether the demand upon the material in parts of the structure might not be increased. The question became still less clear when it was observed that parts of structures, based upon these generally accepted values, suddenly broke after some years without any assignable cause. These facts led Wöhler to make his experiments on the repeated straining of material, and as a result of these he enunciated the following law: "The fracture of material may be brought about by repeated oscillations of strain, no one of which reaches the absolute breaking limit. The difference of the strains which are the limits of the oscillations gives the measure of the destruction of the cohesion of the material. The greater the difference of the strains, the less is the value of the upper limit of strain which will bring about the fracture of the material." Wöhler's researches showed that for every given difference of strains there is a maximum strain at which fracture will not take place with constant repetition of the varying strains. This maximum strain the Author calls the oscillatory strength of the material. The greater the difference of strains the smaller is the

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oscillatory strength, and consequently the smaller the permissible strain.

Many attempts have been made to reduce Wöhler's law to formula. The simplest is that known as the Launhardt-Weyrauch formula (from its two originators). By empiric methods the following expression for the oscillatory strength of wrought iron is deduced from the experiments:—

$$s = 22\left(1 + \tfrac{1}{2}\frac{\text{min. S}}{\text{max. S}}\right),$$

the units of measurement being the kilogram and millimetre. Here min. S is the smallest strain, max. S the greatest strain. If both strains are of the same kind, $\frac{\text{min. S}}{\text{max. S}}$ is positive; if opposite, negative.

If min. S = max. S, s = 33 kilograms per square millimetre,
= 20·95 tons per square inch.
If min. S = 0, s = 22 kilograms per square millimetre,
= 13·97 tons per square inch.

If min. S = max. S only, a dead load is in question, and s is the same as the generally assumed breaking-strength of the material. This the Author proposes to call the bearing-strength (t); s has its least value when min. S and max. S are equal in amount and opposite in kind; then

s = 11 kilograms per square millimetre,
= 6·98 tons per square inch.

In applying this formula to the determination of the cross-section of structures, it is to be borne in mind—

i. The repeated applications of the normal loads on which the calculations are based must not produce fracture.

ii. The strains caused by this loading in the different parts must never reach the limit of elasticity of the material.

iii. As great security as possible must be provided against failure of the structure in the case of exceptional demands such as do not enter into the calculation made upon it. For example, a train leaving the rails when upon a bridge.

Conditions i. and ii. refer to the normal load, which has now to be determined.

Condition i.—In bridges the live load causes little shocks, which increase the demand upon the structure. To allow for this, Gerber multiplies the actual load by 1·5, and Winkler by 1·3. The Author considers that different coefficients should be used for different parts of the structure; thus, for portions where the greatest strain is produced by the load on one wheel, or on one pair of wheels, he considers that the demand upon the metal is twice what it would be if the same load were applied as a dead weight,

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whereas if the greatest strain is produced by the loads upon a number of wheels, the coefficient of shock is smaller, as the shock from all the wheels will not be simultaneous. He proposes the following:—

Number of Wheels.	Coefficient of Shock.
1	2·0
2	1·8
3	1·6
4	1·5
5–6	1·4
7–9	1·3
10 or more.	1·2

These coefficients cannot, of course, be taken to be exactly accurate, but the use of them will bring the result nearer the truth than if a uniform coefficient is used.

In addition to the calculated principal strains in the members of a structure, there occur secondary strains, whose amount is ascertained partly by theoretical considerations, and partly by experiment. It appears to be generally correct to take the highest limit of these at 33 per cent. of the principal strains. The strain must therefore be so provided for that, if it is increased by 33 per cent., the oscillatory strength of the material is not reached. Bearing this in mind, the permissible strain for wrought-iron will be,

$$k = 16\left(1 + \tfrac{1}{2}\frac{\text{min. S}}{\text{max. S}}\right).$$

As, however, so little is yet known as to the exact limit of the oscillatory strength, it is prudent to diminish this still further, and to take

$$k = 12\left(1 + \tfrac{1}{2}\frac{\text{min. S}}{\text{max. S}}\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

which gives the permissible demand upon the metal as from 6 to 18 kilograms per square millimetre (from 3·8 to 11·4 tons per square inch), according as the variations in the strains is great or 0.

Condition ii.—The limit of elasticity for wrought-iron may be taken at 16 kilograms per square millimetre (13·16 tons per square inch). Assuming that the actually occurring strains are 33 per cent. greater than those calculated, the limit of permissible strain is 12 kilograms per square millimetre (7·62 tons per square inch). It has hitherto been usual to attempt to unite Conditions i. and ii. by depressing the value got from Condition i., until its upper limit satisfied Condition ii. This gives, if 12 kilograms per square millimetre is taken for the upper limit,

$$K = 8\left(1 + \tfrac{1}{2}\frac{\text{min. S}}{\text{max. S}}\right),$$

the permissible strain varying between 4 and 12 kilograms per square millimetre (2·5 to 7·6 tons per square inch). This gives

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too small a value for the lower limit, whereas the upper limit is high enough. The Author therefore proposes to use equation (1), and to strike out all values above 12 kilograms, on account of the limit of elasticity. It follows from this that (1) can only be used for calculating the permissible strain when $\frac{\text{min. S}}{\text{max. S}}$ is negative, that is, when the member in question is subject to alternate tension and compression.

The practical rules resulting from these considerations are:—

(*a*). The live load is to be multiplied by a coefficient of shock, depending on the number of wheels which carry the load.

(*b*). If the member is exclusively in tension, or exclusively in compression, the permissible strain is 12 kilograms per square millimetre (7·6 tons per square inch).

(*c*). If the member is subject to alternate tension and compression, the permissible strain must be got from equation (1).

In regard to Condition iii., it is usually impossible to calculate the effect of such shocks upon the structure, even if the point at which they may occur is known generally. All that can be done is so to choose the material that it shall have great power of resistance to shock.

The capacity of work of a bar of cross-section 1 and length 1 is represented by the area of a figure bounded by a curve, whose abscisses are the extensions produced by the strains represented by the ordinates. Professor Intze has obtained for this area of work in a bar of cross-section 1, and length 1, the expression—

$$A = \frac{\delta}{3}(g + 2t) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where—

g = strain at limit of elasticity;
t = breaking strain;
δ = extension at fracture.

For length l and cross-section F this becomes—

$$F\,l\,A = F\,l \cdot \frac{\delta}{3}(g + 2t) \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Suppose a bar hung vertically, with such a weight P attached that the limit of elasticity is just reached; the extension is very small, and may be neglected, in comparison with the extension at fracture. Now let a load Q, moving in the direction of the length of the bar from the fixed end, come upon the free end of the bar at a velocity due to a fall h, and, in consequence, the bar be extended by a length δl to the breaking limit.

The work done by external forces is—

$$Q\,h + (P + Q)\,\delta\,l,$$

and this must be equal to the capacity of work (3). Hence—

$$Q\,h + (P + Q)\,\delta\,l = F\,l \cdot \frac{\delta}{3}(g + 2t),$$

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which gives the necessary cross-section of the bar as—

$$F = 3\,\frac{Q\left(1+\frac{h}{\delta\, l}\right)+P}{g+2t} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

If the height of fall were given by the nature of the work, this formula might be used for calculating the cross-section of the bar. Such a case, perhaps, occurs in the case of ropes in shafts. The cross-section, for security against shock, must be at any rate greater than F as got from (4). Also, of course, $\frac{P+Q}{F}$ must not be greater than k as obtained from conditions i. and ii.

Again, putting $\frac{P+Q}{k}$ for F in (4),

$$h = l\,\frac{P+Q}{Q}\cdot\frac{\delta}{3\,k}\cdot(g+2t-3k),$$

which gives the height of fall required to produce fracture.

If two bars of equal length and cross-section, but of different material, are considered, that material is to be preferred for construction which admits of the greatest height of fall h without fracture. Omitting in the above expression for h the factor $l\,\frac{P+Q}{Q}$ which is common to the two materials, the value—

$$Z = \frac{\delta}{3\,k}\,(g+2t-3k) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

is obtained, which is a measure of the usefulness of the material.

It is clear that A (equation 2) is not by itself a measure of the usefulness of a material. If it were so, a very soft material, with low breaking-strength, but great extensibility, would be a better material than one with a higher breaking-strength and small extensibility; for example:—

E or wrought-iron g = 18 kilog.	For copper g = 6 kilograms,
t = 38 „	t = 20 „
δ = 15 p.cent.	δ = 36 per cent.,
and hence A = 470	and hence A = 552

which would show soft copper to be a better material than wrought-iron. It is clear that the capacity of work (A), though of great importance, is not the only criterion of value of the material. The following is, however, true for all parts of a structure which undergo tension only, or compression only: of two materials, with equal specific capacity of work, that is the better which has the highest limit of elasticity and breaking-strength; also of two materials with the same limit of elasticity and breaking-strength, that is the better which has the greatest capacity of work.

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It will be seen that the expression Z (equation 5) satisfies both these conditions.

The above applies only to bars subject to tension; the question becomes more difficult for bars liable to buckling. Here great extensibility is disadvantageous. Taking the ordinary formula for buckling, the Author deduces for compression-bars an expression for the value of the material similar to Z (equation 5) for tension-bars, from which it appears that there is a certain value of δ (extension at fracture) at which Z is greatest, and that variations in the value of δ are of much less importance in compression than in tension.

W. B. W.

The St. John Bridge, New Brunswick.

(Scientific American, Nov. 28, 1885.)

The St. John's river, before it enters the Bay of Fundy, is contracted between two solid ledges to a width of about 500 feet. Here the railways of New Brunswick and Nova Scotia approach each other, and the traffic of passengers and goods, which was hitherto transferred from one to the other by means of a common ferry—sufficient funds for the construction of a bridge not having been available—passes now over the bridge. The tide rises and falls 22 feet under the bridge, producing a current of great violence. From this fact the conclusion is drawn, that a cantilever or a suspension bridge was the only reasonable type for the locality. A cantilever bridge resting on high stone piers was built. The rail-level is, according to scale, 75 feet above high tide. The principal opening of the bridge is 477 feet long, whéreas the length of the main structure is 812 feet 6 inches. One end of the structure is approached by a viaduct 400 feet long, in eight spans of much lighter construction than the main structure. The latter consists of two double-armed cantilevers, the one with 191 feet, the other with 143 feet 6 inches length of arm. The distance of 143 feet 6 inches between the ends of the cantilevers, being the remainder of the 477 feet span, is bridged over by a truss of 27 feet depth, while the depth of the cantilevers over the supports is 80 feet and 65 feet respectively. The upper chords of the cantilevers rise from their ends at an angle of about 1 to 1 in the first panel of 24 feet in length, and then at an angle of about 1 to 3 to the towers; the lower chords are horizontal.

The middle portion is suspended from the cantilevers by means of pendulums 27 feet long, slotted holes being provided for the expansion. The expansion of the back-arms of the cantilevers takes place on rollers, the anchorage bars swinging on their lower pivots.

The arms were supported on staging, a derrick, 100 feet high,

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being moved on a temporary railway of 14 feet gauge. This derrick was then used for lifting a double-armed travelling-crane of 36 feet arm-length, and of a width equal to that of the bridge, to the top of the towers, and was then removed. The river-span was erected by means of the travelling-crane, which was fixed to the top of the vertical post of each panel of 24 feet successively, until the centre of the middle girder was reached. The process was then repeated from the opposite shore. The connection between the cantilever ends and the middle girder was temporarily made a fixed one, but with adjusting screws, so that each half of it could be moved in or out for the purpose of making a connection in the centre without a special centre piece.

The erection of the superstructure was accomplished in ninety days.

Some further dimensions are:—

	Feet.	Inches.
Width (uniform) between centres of trusses . . .	20	0
Depth of floor beams	3	0
„ longitudinal stringers	2	6

The calculation of strength was based on the assumption of a moving load of 1¼ ton per lineal foot (= 2,500 lbs.) and a wind-pressure of 40 lbs. per superficial foot.

Wind-bracing is placed between the lower chords, between the upper chords, and at 27 feet above the lower chord.

The superstructure was constructed by the Dominion Bridge Company at Lachine, Quebec, and designed by their engineer Mr. T. Abbott, C.E.

The total cost of the bridge is stated to be 550,000 dollars (say £110,000).

M. A. E.

The Water-Supply of Antwerp during the Summer of 1885.

(Report made to the Town Council by a Committee of Experts.[1])

The long-continued drought of the summer of 1885, and the sudden increase in the consumption of water caused by the International Exhibition, had the effect of preventing the purifying-apparatus from completely destroying the marshy taste and smell which characterise the waters of the Nethe.

The dread of cholera, which at that time was ravaging Spain, rendered the inhabitants of Antwerp anxious as to the wholesomeness of the water-supply, and caused the Town Council to appoint a commission of five chemists, with Mr. C. Angenot, chemist to the City of Antwerp, at their head, to institute a searching investigation into the whole subject.

The report, which was published on the 30th November, treats successively of the choice of a source of supply, of the quality of

[1] The original is in the library Inst. C.E.

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the waters of the Nethe, of the process of purification and its theory, of the general arrangement of the works, of the quality of the water furnished during the dry season of last summer, of the cause of the marshy taste and smell, of the present quality of the water-supply, and finally, of the examination of the water with reference to microbes and bacteria.

The general conclusions arrived at are, that the water supplied by the water company was never unwholesome, that the cause of the taste and smell, which only lasted for a short time, was the exceptional demand for water coinciding with an abnormally bad condition of the river caused by the prolonged drought, and that all doubts about the efficacy of the purification of water by means of iron applied through the instrumentality of the revolving purifiers must fall before the researches of the English scientific men who have investigated the subject,[1] and before the labours of the commission, which has confirmed their conclusions to the fullest extent.

A further report is promised, in which the investigations into the microbe and bacterial life will be more fully discussed.

W. A.

The Naples Waterworks.

(Giornale del Genio Civile, 1885, p. 523.)

The supply is derived from water-bearing strata in the hills, at a distance of about 52 miles from the city, and at an elevation of 1,050 feet above the sea. The water is found in a bed of sand, gravel and chalk, from 10 to 13 feet in thickness, overlying an impervious bed which is found at a depth of about 42 feet below the surface. Above the sand and gravel is a clayey stratum which keeps out surface-water. In places the water comes to the surface in large springs, which are collected in masonry channels. Three culverts have been constructed in the water-bearing stratum, or just below it. These culverts were executed in open cuttings, which were made wide enough for the masonry to be surrounded with dry flints, which were carefully washed before being used. Holes were left at intervals in the sides of the culverts for admitting the water. Above the dry flints the whole width of the cutting was covered with two layers of clay well rammed, with a layer of hydraulic lime in powder between them. Above the clay the cutting was filled in with earth. The combined length of these collecting-culverts is 2000 feet, and they drain an area of 7½ acres. They discharge into a chamber of about 40 feet square, from which the water passes into a main conduit, an overflow being provided to carry off any excess.

This main conduit is 37 miles in length. The country through

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[1] Minutes of Proceedings Inst. C.E., vol. lxxii., p. 24; vol. lxxxi., pp. 279 and 285.

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which it passes varies in character, from the hardest rock to the most plastic clay. It is a covered masonry channel, but the water is not under pressure. The minimum depth of earth over it is 3½ feet. Its internal dimensions throughout the greater part of its length are, 5 feet 3 inches in width, and 6 feet 7 inches in height. The sides are vertical, the invert elliptical, and the arch semi-circular. The section is varied to suit the different soils through which it passes. When it is in tunnel through ground exerting great pressure it is made circular, 6 feet 7 inches in diameter, and where it is carried on a high aqueduct its width is diminished to 4 feet 9 inches, and its height increased to 7 feet 4 inches. The volume of water conveyed is 70 cubic feet per second, the gradient is 1 in 2000. The actual difference in level between the two ends would give a considerably steeper gradient; in fact, there is an excess of fall amounting to 256 feet. Three falls are therefore introduced in the conduit, one of 16 feet, another of 118 feet, and another of 122 feet. These falls are arranged in steps, the treads of which are inclined upwards so as to diminish the velocity. In other respects the conduit is of the ordinary section at these falls. Arrangements have been made at each of these places by which the fall may be utilized as a motive power at some future time by being diverted into pipes instead of tumbling over the falls.

In the course of the conduit there are several tunnels, one 2 miles in length, and their combined length being 9 miles. There are also twenty masonry aqueducts, having a total length of nearly 2000 yards. There are also two syphons of considerable length, each composed of three 31½-inch cast-iron pipes laid side by side, controlled by sluices at each end, and charged by means of a 6-inch pipe laid from the upper end to the lowest point. Valves are provided at the bottom for emptying the pipes. The conduit is divided into twelve lengths of about three miles each, and at the end of these lengths is a sluice-chamber, by which the water can be run off when repairs are required. At intervals of 220 yards (except in tunnels), man-holes are provided for inspecting the conduit, and at each man-hole is a catch-pit for intercepting any deposit which may be carried down by the water. Houses are provided at intervals for the men employed to inspect the work.

From the termination of the masonry conduit the water is conveyed to Naples in three cast-iron pipes under pressure. The whole of the supply brought down by the conduit is 38,000,000 gallons per day. The volume required for the supply of the town is, however, only 22 million gallons, which is at the rate of 44 gallons per head. It is this quantity that is conveyed by the pipes, the remainder being available either for motive power or for future supply. At present it is run to waste. Two of the pipes are 31½ inches in diameter, the third 27½ inches. The thickness of the pipes varies with the head, of which the maximum amount is 600 feet, the 27½-inch pipes in this case being 1·46 inch thick. The total length of pipes laid from the masonry conduit to the service reservoirs is 14 miles of 27½-inch, and 23¼ miles of 31½-inch.

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In addition, an 8-inch pipe was laid from the summit to the lowest point for the purpose of charging the pipes. A telephone-wire was laid along the pipes, so that in case of repairs instructions can be communicated to the men at any point.

In the neighbourhood of the city are two large reservoirs, one at a high, the other at a low level. These reservoirs are excavated in the volcanic tufa rock. That at the low level consists of five parallel tunnels about 160 feet below the surface of the ground. The section of each tunnel is ovoidal, struck from five centres, the bottom being flat; the height is 35 feet, width 30 feet, and the thickness of rock between the tunnels 30 feet. Their total capacity is 17½ million gallons. In places where there are cracks in the rock, a masonry lining is put in, but the greater part stands without any masonry, and is simply rendered in cement up to 18 inches above top water-level to make it water-tight. This rendering was 2 inches thick at the bottom, reduced to ½ inch at the top.

The Paper is illustrated by drawings, and many details of the works are given, but there is no information in regard to their cost.

W. H. T.

Repair of the Dam at Holyoke, Mass.

(The American Engineer, vol. x., 1885, p. 155, 1 woodcut.)

The first dam across the Connecticut river, at Holyoke, was built, in 1848, of timbers fastened together, and to the rocky bed, by bolts. It was, however, swept away; and the existing dam was constructed in 1849. The rock was excavated to a depth of 6 feet; and the dam, 1,017 feet long, was then built up by a series of parallel timbers 6 feet apart, in alternate layers at right angles, to a height of 30 feet. The spaces between the timbers were filled with gravel and stone to a height of 10 feet, and the upper surface of the dam was covered with 6-inch planking, the crest being protected with ⅜-inch plate-iron. An apron, 50 feet wide, was formed in 1868, in front of the dam, of round timbers crossed, the interstices being filled up with stone, to protect the dam from being undermined. The dam, however, has frequently leaked considerably, breaches having been formed in the planking and through the timbers, threatening its destruction. Accordingly a thorough repair was undertaken in 1885, by overhauling the dam in sections of 100 feet at a time, the water being excluded by cofferdams. The site for the cofferdams was cleared of sand, gravel, and *débris* by a powerful jet of water, and the planking was sufficiently stripped off the dam to admit the coffers, which were weighted with stone; and the leakage soon emptied the enclosed space. A row of 3-inch sheet-piling was placed against the back of the cross-timbers of the fourth up-stream row of well-holes between the crossed timbers. Gravel was then tipped into the well-holes, beginning

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next the crest of the dam, and forced into the interstices between the timbers, by three jets from hoses communicating with the river above. The sheet-piling and the timbers of the dam have thus been encased in wet gravel. The dam has been re-covered with two layers of 6-inch planking at right angles, and the rounded crest is protected by ¼-inch plate-iron, 6 feet wide. These works serve to prevent breaches in the dam, to diminish leakage, and to preserve the timber-work by the casing of wet gravel.

L. V. H.

The Use of Lead Pipes for Water-Supply.

By E. REICHARDT, of Jena.

(Deutsche Vierteljahrsschrift für öffentliche Gesundheitspflege, vol. xvii., 1885, p. 565.)

The Author alludes to the recent work of A. Hamon, entitled "Hygiène publique," which advocates the prohibition of the use of lead for water-pipes. This metal is much employed in Germany for the distribution of drinking-water, and it becomes important that a conclusive series of experiments should be undertaken to determine the validity of the objections urged against its use for this purpose. In the year 1879, the Author published in the "Archiv der Pharmacie" some observations upon this subject. He examined the leaden pipes which had been in use for upwards of three hundred years for the supply of water to the town of Andernach, and found them to be coated internally with a layer, about ½ millimetre in thickness, of phosphate and chloride of lead, with a little free oxide of lead. Very small quantities of lime and magnesia were present in this coating, and the metal of the pipes, after being in use for this long period, was perfectly good in quality. Further experiments were made with a piece of common lead piping, which was capable of containing about 1 litre of water, and which was tested alternately with spring water, distilled water, and water impregnated with carbonic acid. Each kind of water was allowed to remain in the pipe for periods varying in length up to several weeks. Both distilled water and water containing carbonic acid speedily gave indications of reaction on the lead; but spring water, even after many weeks, had no action upon the lead. A change from spring-water to distilled water, however, immediately showed signs of a reaction. In all these experiments the pipe was completely filled, so as to exclude the air; but when the pipe was alternately emptied and filled, so as to give opportunities for the oxidization of the lead, the metal readily became soluble in water. The conclusions formed by the Author at that time were, that lead pipes which are not constantly kept filled with water should in every case be rejected for pumps and for water-carriage purposes. Hamon brings forward a long series of experiments to prove that water in which lead was dissolved had produced serious illnesses in France. Orfila, who also treats

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on this subject, was evidently acquainted with the danger of atmospheric action on lead pipes used for water. Reference is made to the investigations of Gautier, who found in the sediment of water which had passed through lead pipes 73·96 per cent. of lead, 8·45 per cent. of phosphoric acid, 1·25 per cent. of chlorine, and traces of several other metals, as also of lime and magnesia. This would indicate the formation of very similar substances to those found by the Author on the Andernach pipes.

Numerous analyses of the amount of lead found in various waters after passing through lead-pipes are given, and the method in which the lead was estimated is described. The Author explains how his tests differed from those of the French chemists, and points out the reasons for his opinion that his results are more reliable. In his case he precipitated the metal directly from the water itself, whereas in the French experiments the water was acidified, evaporated to dryness, and the residue examined for lead. He points out that his investigations appear to establish the fact that all the dissolved lead is abstracted by boiling the water. In conclusion he states that all the authorities concur in their estimate of the relatively very small amount of lead contamination in ordinary drinking-water which has passed through leaden service-pipes; the use of open leaden vessels, or leaden channels in which air can enter, should, however, be prohibited.

G. R. R.

The Destructive Action of Cement Mortar upon Lead Piping.

By Otto Peschke, of Berlin.

(Gesundheits-Ingenieur, 1885, p. 607.)

Owing to a communication from an engineer at Hanover, the Author forwarded a sample of lead pipe, which had served for about six years as the supply-pipe to a fountain-basin, to Dr. v. Knorre at the chemical laboratory of the Berlin Polytechnic for analysis. This pipe, throughout the portion of its length which was embedded in the cement, was powerfully corroded; the corrosion being most marked at the end of the pipe next the basin, and the effect diminishing as the pipe receded from the water. The pipe was coated with a chocolate-coloured layer of oxide of lead, of the hardness of glass, which adhered strongly to the metal. A portion of the brownish mass, at that part where it was the thickest, was detached from the pipe, pounded in an agate mortar, dried at 110° Centigrade, and submitted to chemical analysis. It contained 99·05 parts of oxide of lead, the residue consisting of carbonic acid, with traces of lime and silica, which latter may have been due to small quantities of cement adhering to the outer surface of the pipe. Dry Portland and Roman cements have been found by the Author to have no action upon lead, but the presence of water appears

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necessary to effect corrosion. The Author invites the attention of engineers and chemists to this action, which is difficult to explain, and which may be due to certain phenomena in connection with the induration of cement mortar which are at present extremely obscure.

G. R. R.

Report of the International Commission on the Suez Canal.

By A. FLAMANT.

(Annales des Ponts et Chaussées, 6th series, vol. x., 1885, p. 744, 1 plate.)

The Commission was appointed, in 1884, to determine what new measures, in respect of works and navigation, should be undertaken to enable the ship-canal to meet fully the exigencies of a traffic exceeding 10,000,000 tons per annum. Its Report was presented in February 1885, of which document the Author furnishes a summary. The Commission considered three methods of increasing the carrying capacity of the canal, namely: (1) widening the existing canal; (2) construction of a second canal; (3) doubling the capacity of the canal by a combination of the first two methods. When the canal was first designed, in 1856, it was supposed that two vessels, being towed, could easily pass where the bottom width was 144 feet, or double the normal width adopted. At the present day, however, when vessels of 50 feet in width propel themselves through the canal, a bottom width of 230 feet has been proposed for the 81 miles from Port Said to the southern end of the Bitter Lakes, where the tidal currents do not exceed 1 knot an hour, and 262 feet for the rest of the distance to Suez, where the currents often exceed 2 knots, in order that the vessels may pass each other freely. The cost of this widening was estimated at £8,240,000, supposing the depth of the canal remained as at present, 26¼ feet below low-water of ordinary spring tides, but would be increased by £975,200 if the depth was augmented to 29½ feet, unless the proposed width could be reduced 18 feet. The construction of a second canal, within the limits of the company's lands, having, like the existing canal, a bottom width of 72 feet, widened out to 131 feet through the small Bitter Lakes, was estimated at from £8,200,000 to £8,920,000, with an additional cost of £698,800 if made 29½ feet deep. The third plan took into consideration the different velocities of the tidal currents north and south of the Bitter Lakes. Assuming that the greater velocity might lead to collisions between vessels passing on a single enlarged canal, it would be advisable to restrict the enlargement to the northern portion, and to form a second canal between the Bitter Lakes and Suez.

The Commission decided unanimously in favour of the enlargement of the existing canal from the Mediterranean to the Red Sea,

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for the following reasons. An enlarged section would enable vessels to increase their speed from 5½ to 8 knots an hour, and thus to traverse the canal in about twelve hours, which could never be accomplished with two separate canals; and, moreover, there would be only two banks to maintain, instead of four. This increase of speed will greatly facilitate the steering, which, together with the greater width of canal, will enable vessels to avoid stranding on the banks, an important gain which would not be obtained with two canals. The danger of collisions between passing vessels on a single canal will be obviated by the great increase in width proposed for the canal, and by reducing the speed of the vessels in the act of passing. Moreover, the plan of enlargement will include the easing of the curves on the canal, and will thus remove the impediments which these sharp and narrow bends present to vessels of 360 feet in length, which is quite an ordinary length now, though rare twenty years ago. Lastly, this system will possess the inestimable advantage of enabling each successive portion of enlargement to be at once utilized as an addition to the passing places for vessels.

The Commission exhibited some difference of opinion on the question whether, in order to keep within the lower estimate, the depth should be increased at the expense of the width; but it was eventually agreed that the depth should be increased to 28 feet, with a corresponding decrease in the proposed width of the northern portion to 213 feet, and of the southern portion to 246 feet, measured at a depth of 26¼ feet. The present limit of draught is 24½ feet; but it was considered necessary to provide for a probable increase to a maximum of 27 feet, which a depth of 28 feet would just accomplish, leaving the deepening to 29½ feet for a future time, when the increased number of vessels of large draught may demand it. Whilst adopting the above width for the straight portions of the canal, as sanctioned by the experience of navigation on the Clyde, the Tyne, the South Pass of the Mississippi, and the Sulina mouth of the Danube, with similar widths and more rapid currents, the Commission laid down the widths suitable at curves in proportion to their radii. In the northern portion of the canal the widths, 26¼ feet below low-water, were fixed at 246 feet for curves exceeding 8,200 feet in radius, and 262 feet for sharper curves; whilst for the southern portion, the widths at the curves, which all exceed 8,200 feet in radius, were designed to be 262 feet. As the long slopes of the canal banks, where unprotected by vegetation, are seriously damaged by the wash of passing vessels, the formation of a narrow berm on each slope, below the action of the waves, about 6½ feet below the water surface, has been recommended, and also the protection of the slopes from that depth up to 3¼ feet above the water by pitching. Owing to the increased speed admissible on the enlarged canal, the normal period of transit will not exceed twelve and a half hours, allowing ample margin for stoppages; so that the journey might be accomplished in a single day, if an early start

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was effected so as not to be overtaken by nightfall; whereas at present the average time occupied in passing through the canal is forty hours. The Commission considers that the works should be executed in three distinct stages, namely: (1) An adequate increase in width, to provide for the passing of vessels at any point of the canal, by stopping one vessel and drawing it to the side, together with an increase in depth to 28 feet, at an estimated cost of £2,449,750; (2) the completion of the enlargement to the full width prescribed, at a cost of £5,190,900; (3) the deepening of the canal throughout to a depth of 29½ feet, at a cost of £618,050, making a total estimated cost of £8,258,700 for the whole of the works. Adding, however, £40,000 for sundry expenses, and deducting £180,100 for sale of plant, the final estimate is £8,118,600.

L. V. H.

Swedish Canals. By E. Roloff.

(Zeitschrift für Bauwesen, 1886, p. 57.)

This is a description of the most important group of canals in Sweden, being those connecting the lakes Wener and Wetter (2124 square miles and 733 square miles respectively in area), together with many of the smaller lakes, with the sea, and forming a navigation from the North Sea to the Baltic, thus providing a short route for vessels trading between Stockholm and Gothenburg, and the avoidance of the dangerous navigation of the Sound.

The canals forming this main route are three, viz.:—The Trolhätta, the West Göta, and the East Göta. The first-mentioned navigation extends from Gothenburg to the Wener Lake, utilizing for the greatest part of the distance the river Göta-Elf, the canal works being limited to those places where the main stream is non-navigable on account of waterfalls, or rapids. The West Göta canal connects the Wener with the Wetter Lake, crossing the intervening watershed at the small lake Wiken. The third section is the East Göta canal navigation, which on leaving the Wetter Lake passes through two smaller lakes, Boren and Roxen, and terminates by a junction with the Gulf of Stätbacken, on the Baltic, about 70 miles south of Stockholm.

Besides the above, there are two tributary canals described, viz.: the Dalsland, and the Kinda. The Dalsland Canal connects a series of small lakes (Hjerterud, &c.), lying to the north-west of the Wener Lake with the latter; and the Kinda Canal gives access from a chain of four small lakes (Asunder, &c.) stretching southwards; to the East Göta Canal, where the latter passes through the Roxen Lake.

Most of the vessels using the main canals are built for coasting, and reach a tonnage of 250, and the steamers average 110 tons and 60 HP.

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At Trolhätta, about 1¼ mile below the point where the river Göta-Elf leaves the Wener Lake, there occurs a series of falls and rapids, the river descending 108 feet in a length of about 4,590 feet. A description is given of works which were commenced at this place early in the last century and were well advanced in 1755, when an unusually heavy flood caused much destruction and loss of life, and the abandonment of the works, never since resumed. The intention was to surmount the difference of level, viz., 108 feet, at the Falls above mentioned, by three locks only, with a rise of 36 feet each. In the canal, as constructed in 1800, there is a chain of eight locks (still in service), but these being insufficient for the traffic, a second set of eleven were constructed alongside the former in 1844. These are cut in the solid granite. There is a total of sixteen locks with a fall of 142 feet on this canal (Trölhatta), which is 22 miles long. The breadth of the canal-bottom is 39 feet in soil and 23 feet 5 inches in rock, with a depth at mean water-level of 12 feet 8 inches. The number of vessels passing annually is 7000.

The West Göta Canal connecting the Wener and Wetter Lakes rises from the former lake by a series of nineteen locks, or a height of 154 feet 6 inches to the summit level, which is 300 feet above the sea, and the descent from here to the Baltic *viâ* the East Göta canal is by thirty-nine locks. The breadth of the bottom of these canals is 46 feet 9 inches, with a mean depth of water of 9 feet 9 inches. These two canals were completed in 1832 at a cost of £887,500, the length of navigation being 116⅔ miles, of which 54⅓ miles is artificial canal, and 62⅓ miles lake channel. The traffic is from 4000 to 5000 vessels per annum.

The Dalsland Canal rises 192 feet 6 inches by twenty-five locks, the summit level being 338 feet above the sea. The length of the navigation is 155 miles; but the actual length of the works effecting this is only 4·8 miles.

The locks on this canal are each about 98 feet 6 inches long, with a breadth of 13 feet 8 inches, and depth over sill of 5 feet 4 inches. The breadth of the bottom is 14 feet 6 inches and 15 feet 7 inches in soil and rock respectively. It is navigated by vessels of 70 tons, and steamers of 45 tons and 25 HP. The traffic amounts to 4000 vessels per annum. It was completed in 1868 at a cost of £81,500.

The Kinda Canal rises 171 feet by fifteen locks to a level of 277 feet above the sea. The length of the navigation is 49½ miles, of which 22¾ is either artificial canal or trained river. The length of the locks is 90 feet 6 inches, breadth 18 feet 4 inches, and depth over sill 4 feet 10½ inches. The traffic is from 3000 to 4000 vessels per annum. It was completed in 1871 at a cost of £72,500.

Descriptions and diagrams are given of the locks, chambers, and gates, rolling bridges, &c.; also of a wrought-iron aqueduct of 108 feet 3 inches span, carrying the Dalsland Canal over a waterfall near Häfverud.

The canals are closed by ice, generally from the 1st December

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to 15th April, but the loss of time thus enforced is partially compensated for by the practically continuous daylight during summer when navigation proceeds day and night.

D. G.

Lift-Bridge over the Ourcq Canal at La Villette, Paris.

By R. AUDRA.

(Le Génie Civil, vol. viii., 1885, pp. 1 and 18, 2 plates and 2 woodcuts.)

The tonnage of vessels entering and leaving Paris amounts, on the average, to a little over 4,000,000 tons; and about 1,160,000 tons of this total belong to the traffic of the Ourcq canal and the adjoining Villette basin in Paris. The Ourcq canal is connected with the Marne, and communicates with the Seine, both above and below Paris, by means of the St. Martin and St. Denis canals. The swing-bridge, which formerly carried the Rue de Crimée across the canal, afforded a clear width for vessels of only 25½ feet; whilst the depth of water was only 6½ feet. The improvement of the Lower Seine rendered these dimensions quite inadequate for the traffic; and the Municipality of Paris, who own these important canals, determined to increase the width at this crossing to 50 feet, and the depth to 10½ feet. A large sewer went along under the street, which, besides affording accommodation for the telegraphic, telephonic, and pneumatic services, also contained two conduits; and the summit of its arch reached the old bed of the canal. In order to avoid the introduction of a siphon, on lowering the bed of the canal 4 feet, the sewer was rebuilt under the canal in two branches, with the same bottom-level but reduced in height, one branch serving for the sewer, and the second for the other purposes. This work was effected during the ordinary summer stoppage of traffic on the canal for nineteen days. A high foot-bridge, approached by steps, and a movable road-bridge were erected temporarily for the traffic during the construction of the lift-bridge; for numbers of foot-passengers, about four thousand vehicles, and eighty boats traverse the place daily. A lift-bridge has been adopted, since it necessitates no raising of the approach-roads, as required for an ordinary fixed bridge, to provide the required headway of 17¼ feet for the vessels; nor does it interfere with the approach, as in the case of the long tail of a swing-bridge. A bridge of the same type, but primitive in form, had been previously erected across the same canal.[1] The new bridge consists of a movable platform, borne by iron girders and suspended at its four corners by strong chains, passing over pulleys placed on four cast-iron columns, to the ends of which weights are attached for counterpoising the bridge. The bridge is lifted by two double-acting hydraulic presses, placed at each extremity of its axis; but

[1] Minutes of Proceedings Inst. C.E., vol. lxxxi., p. 355.

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machinery is provided for raising it by hand, in the event of a failure of the hydraulic apparatus. The equal motion of both sides of the bridge is secured by cross-shafts at each extremity, terminated by toothed wheels at each corner, working in a rack running up each column; and the cross-shafts are connected together by a longitudinal shaft, with conical toothed wheels, to equalise the motion at both ends. To provide against accident in the event of the fracture of one of the suspending chains, a lever is placed below each corner of the bridge, with an arm attached to the end of the chain, which, if released by the breaking of its chain, would force the other end of the lever, by a spring, into the rack-grooves of the adjacent column, and thus prevent the descent of the bridge. The length of the bridge is 65½ feet, and its total width 25 feet. The roadway is 16½ feet wide, and the foot-paths, on each side, 3 feet 11 inches. The hollow columns are 25 feet high, and from 2¾ to 2¼ feet in diameter. Each column carries a pulley, 8 feet in diameter, on which the suspending chain rests, having links of 2½-inch iron. Cast-iron disks, 4 feet in diameter, encircling bars at the end of each chain, form the counterpoise weight of 84 tons, which is equal to the weight of the movable portion of the bridge. The total weight of the work is 241 tons, and the total cost was £5,000. An iron foot-bridge has been erected near the lift-bridge to prevent any interruption of the passenger traffic. This bridge is arched in form; it has a span of 78¾ feet, a clear height at the crown, above the water-level, of 22 feet, and a width of 11½ feet between the railings.

L. V. H.

The best Depth of Lock and Position of Upper-Gate Sluices for saving Time in Locking. By F. Barbet.

(Annales des Ponts et Chaussées, 6th series, vol. x., 1885, p. 727.)

Comparative experiments were made at some locks on the Scarpe Moyenne and the Sensée Canal, with the object of ascertaining the influence of the depth of the lower sill below the water-level of the lower pool, and of the position of the sluices in the upper gates with reference to the same level, or the time occupied in locking vessels. The results of the experiments are given in two tables, and are also represented in a formula. When the sluice-gates are high up, they must be opened gradually at first to avoid a great rush of water into the chamber, whilst, if placed with their sills level with the bottom of the chamber, they cause, when lifted, a rush of water under the vessel in the lock, which, rebounding from the lower gates, would drive the vessel, unless strongly moored, against the upper gates. Accordingly, in both these cases, time is lost in filling the lock by the necessity of only partially lifting the sluice-gates at first. The best position is when the sluice-gates are just below the level of the lower pool,

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for then the gates can be fully opened at once, as the rush of water is partially broken against the bows of the vessel, and both reduces and neutralizes the back-current. The commotion is less in proportion as the depth of the lock below the lower pool is greater; but it has been found that an adequate acceleration in the locking is effected by placing the lower sill about 7½ feet below the water-level of the lower pool. This is especially applicable to old locks, where a greater lowering of the sill would not effect such an acceleration in lockage as to compensate for the increased difficulty in securing the stability of the work. Under these conditions, the lock is most rapidly filled by placing the top of the lift-wall 3¼ feet below the water-level of the lower pool, so as just to submerge the sluice-gates. The experiments show that by this arrangement the time of filling the lock, which is eight minutes thirty-eight seconds when the sluice-gates are above the lower water-level, is reduced to five minutes fifty seconds.

L. V. H.

Duration of Lockings and Transit of Boats on the Bourgogne Canal.[1] By H. Bazin.

(Annales des Ponts et Chaussées, 6th series, vol. x., 1885, p. 396.)

The time occupied by a boat in passing through a lock comprises the entrance and exit of the boat, and the operations in locking. By the adoption of sluices in the side walls, the locks on the Bourgogne canal can be filled or emptied in two minutes; but the time employed in taking in and bringing out a boat varies considerably, depending on the speed of the boat, its draught, and its method of traction. Steamboats, carrying from 100 to 150 tons of merchandise, traverse a lock in from 6 to 8 minutes; whilst yachts and a torpedo-boat have passed in 4 to 6 minutes. The main water traffic between Paris and Lyons is carried on by new boats 125 feet long, and having a draught of 4½ feet, being limited by the deficiency in depth of the Yonne. These boats can carry 210 tons, but their load is usually between 130 and 180 tons; and they perform the journey between Paris and Lyons in eleven to twelve days, traversing the Bourgogne canal in six or seven days. Boat-builders often err in constructing boats of the largest size which the locks will admit, which renders the entrance and exit of the boats both slow and troublesome. A boat of 200 tons, travelling 22 miles per day, is more serviceable than a boat of 275 tons, which can only go 12½ miles. The greater speed entails a somewhat greater cost in traction; but it admits of more voyages, the transport of more freight, and a more regular service. The lengthening of the locks on the Bourgogne

[1] Minutes of Proceedings Inst. C.E., vol. lxxx., p. 402.

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canal, by enabling the tonnage to be increased by one-third, without diminishing the speed of transit, or notably increasing the cost of traction, has proved a most profitable work for inland-navigation commerce.

L. V. H.

The Treatment of Sediment-carrying Mountain Streams in Europe, and its application to California.[1] By G. J. Specht.

(Engineering News and American Contract Journal, vol. xiv., 1885, p. 289, 4 woodcuts.)

Atmospheric influences are continually disintegrating the surfaces of mountains, especially where unprotected by vegetation; and the loose material is carried down by the rainfall into the plains below. A torrential stream passes generally through four distinct divisions in its course; namely, the region of erosion in the upper part of its valley, followed by the region of transportation of the eroded materials, which are then left behind in the region of deposition; and lastly the region of discharge, through which the stream flows to the main river. The last division is sometimes absent; and occasionally all four divisions are found in successive portions of the same stream. The aim of improvement-works should be to prevent or decrease the formation of detritus, and to arrest the detritus before it reaches the plains. These objects are attained by encouraging the growth of trees and vegetation on the mountain slopes, which prevent erosion, and by the construction of *thalsperren*, or dams, across the valleys of the torrents, which check the erosion of the bed and banks of the stream by reducing its fall, and retain the detritus in its downward passage. The dams are constructed of rubble stone or of masonry, or even of timber where stone is expensive; or they may be advantageously formed of hurdles or fascines made of green branches, which by driving down roots below, and growing above, yearly increase their strength and size. Greater dams have to be built where large quantities of detritus have to be stored; and sites have to be selected in the valleys with a large storage capacity above the dam. These dams require raising periodically as the detritus accumulates; and eventually, when the site is filled with detritus, trees are planted, and another site is selected for storage. Where the bed of the torrent is very steep, steps of rubble stone have been formed to reduce the slope, and a dam of masonry in mortar constructed at the base of the steps. These works have been successfully applied in Europe; and though the mountain slopes of California have not been equally denuded of vegetation by the destruction of forests for timber and by the stripping of herbage by unrestricted pasturage, detritus is largely produced by washings

[1] Minutes of Proceedings Inst. C.E., vol. lxxi., p. 453.

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from the mines. The Author proposes that a similar system should be adopted for all the Californian river-valleys affected by detritus, according to a comprehensive plan under State control; and he considers that the mines might be allowed to continue their operations, on condition that they should be restricted to a certain maximum yearly output of gravel.

L. V. H.

Method of Navigation applicable to the Rhone between Lyons and Marseilles. By L. JACQUET.

(Annales des Ponts et Chaussées, 6th series, vol. x., 1885, p. 362.)

The rapid current of the Rhone renders its navigation different to that of the other French waterways. The system of the future on the Rhone will be towage; and the Author conducted some experiments, in 1882, with the object of ascertaining the practicability of towage on the Rhone. To place the traffic by water between Marseilles and Lyons in direct communication with the inland navigation of France, canal barges, carrying 300 tons and drawing 6½ feet, would have to be adopted. Such barges, however, would be unsuited for the sea-voyage from Marseilles to the town of St. Louis, and would not find an adequate depth in the Rhone between Arles and Lyons. A canal-barge, therefore, on being loaded at Marseilles, would be floated into the chamber of a specially constructed sea-going vessel, which would convey it to the town of St. Louis in three or four hours, where it would be floated out and towed, in company with another barge, up to Arles by a screw steam-tug. At Arles, the barge would be placed in the chamber of a river-boat, drawing only 3¼ feet when fully loaded; and two of these special boats would be towed up to Lyons by a tug hauling upon an immersed chain. At Lyons, the barge would be floated out again, whence it could continue its voyage on the Saône and the canals. In descending the Rhone, the barge would only be half loaded; and the river-boat, bearing it down to Arles, could float down with the stream, or obtain the help of a descending tug not using the chain. A vessel of 200 HP. could convey the barge between Marseilles and the town of St. Louis; and the small draught required for the river-boat could be realized by making the boat about 30 feet broad and 200 feet long. Each river-boat, by saving two trans-shipments, would effect a reduction of cost of £1,065 in a year; whilst its total yearly expenses, including capital charges and maintenance, are estimated at £520, making an actual saving of £545 per boat. A series of experiments were carried out during the whole of the year 1882, in order to ascertain the importance of the objections raised against the adoption of towage on the Rhone,

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and the best system to employ. The objections urged were: the danger resulting from the fracture of the chain; the danger of collisions; the burying of the chain in the gravel-bed; the wear of the chain, and its displacement at curves. No fracture occurred during the experiments; but it was found that the steam-tug could keep its train of boats in the proper course (even when the chain was suddenly thrown overboard) by means of its paddle-wheels, which commenced working at once. The chain, on passing the haulage-drum, goes through a ring to which a cable is attached, with a buoy at the other end. The chain, therefore, on being thrown into the river, carries the buoy along with it, which serves to mark the position of the chain, and enables it to be picked up within fifteen to twenty minutes. On the approach of any vessel, the tug can easily let go the chain and direct its boats, if necessary; so that all danger of collision from the use of the chain is avoided. The experiments, moreover, demonstrated that the release and recapture of the chain was so simple, that, when once the chain has been laid down, it might be freely used by all suitably fitted tugs. No serious difficulties were experienced, during the experiments, in raising the chain when buried in the shifting gravel of the Rhone bed; and when the works for fixing the river-bed have been completed, it may be assumed that no places will offer greater difficulties than those already surmounted. Longer experience would be necessary to judge of the rate of wear of the chain; but no wear was perceptible at the close of the experiments, though the roller on the tug had to be renewed. The displacement of the chain at curves would be very gradual and slight, and could easily be rectified by the passage of a small tug along it. The Author then discusses the relative advantages of an immersed chain all along the river, and relays of tugs hauling on their own cables, which they lay down on their descent. Though the cost of the cables would be less than the chain, they would wear out more rapidly; and the tugs would have to carry a varying load in winding up their cables. Moreover, with an immersed chain, the tugs could be provided as the traffic increased; whereas, with relays, the tugs would have to be all provided at once, and about thirty would be required of about 200 HP. each, and the breaking of a single relay would stop the traffic. The preference, accordingly, is given to an immersed chain, which would be more convenient for working, and more economical than relays, except with a large traffic.

The system of towage indicated above would enable freights to be carried between Lyons and Marseilles at a rate of 5*s*. 8*d*. per ton; and as the distance between these places by water is about 233 miles, the cost of transit would be under 0·3*d*. per ton per mile.

L. V. H.

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Notes upon the more Important Features of Large Reservoirs for Irrigation. By G. Torricelli.

(Giornale del Genio Civile, 1885, p. 611.)

The Author, having recently had to visit and study a large number of reservoirs, has noted some points which he thinks deserving of attention.

The weight of masonry dams has generally hitherto been taken at 125 lbs. per cubic foot in calculating their stability. This is too little, and from 137 to 143 lbs. is the weight usually adopted now. On the other hand 62·4 lbs. per cubic foot is still taken as the weight of water, whereas the Author's experiments have clearly demonstrated that turbid water, such as is often found in reservoirs, weighs from 63 to 75 lbs. per cubic foot. While therefore 62·4 lbs. may safely be taken for cases where flood-waters are excluded, a higher value should be adopted where these are admitted to the reservoir. It has been found on calculating the stability of certain dams, taking 68·6 lbs. as the weight of water, that the pressures and tensions at the external and internal faces of the dams are from one-fourth to one-third greater than when 62·4 lbs. is adopted.

Three dams in Algeria have recently failed, the Habra [1] in 1881 and the Cheurfas and Sig dams in February, 1885. At Hamiz the water was admitted into the reservoir in 1884, but the dam having shown symptoms of failure, the water was at once run out, and an immense retaining wall was built to arrest the movement. The water was admitted into the Cheurfas reservoir in January, 1885, and it at once began to make its way through the permeable ground at one end of the wall. The sluice at the bottom of the reservoir could not be opened, and after a time a large quantity of earth was washed away, carrying with it some 30 feet of the dam. The water washing through this great aperture caused a flood in the river below. Some distance down stream the Sig reservoir was situated, and the flood pouring down topped the dam by 18 feet and overthrew it. When the Hamiz dam failed it was subject to a pressure of 157 lbs. per square inch and a tension of 43 lbs. per square inch, which the Author considers too high for mortar, which, though of excellent quality, had not had sufficient time to set. The Author points out that a slight increase in the height of the water in the reservoir above that for which a dam is calculated produces greatly increased strain upon the work, and therefore means should always be adopted to ensure that the water will not rise above the intended level, and he recommends that dams should be built circular on plan, so as to act as arches, particularly if the foundations are not very solid, and instances that of Chéliff, which

[1] For an account of this dam and of its failure, see Min. Inst. C.E. vol. xli. p. 248; vol. liii. p. 325, and vol. lxx. p. 447.

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is 59 feet high, and has twice had water running over it to a depth of 13 feet. This dam is founded upon beds of sandstone, alternating with clays, which formed a very unreliable foundation; nevertheless the dam has never shown any sign of failure, though subjected to severe compressive and tensile strains, and the explanation of this fact is that the dam is formed in an arc of a circle of 492 feet radius, the length of the arc being 260 feet.

The highest earth dam in the world, with the exception, perhaps, of some in India, is said to be that of Oued Meurad, in Algeria, which has a height of 95 feet. This was completed in 1864, and is in very good condition. A few fissures have appeared at the top and have been filled in with masonry. The bank stands upon a bed of basaltic rock, no excavation having been made to receive it. About 3 gallons of water per second leak through, but without endangering the bank. It was constructed in layers, but these, instead of being horizontal, were normal to the outer slope. When the bank had been completed up to a certain height, the reservoir was allowed to fill; this caused the bank to settle down, and when it stopped settling the work was resumed and the bank completed. This is a somewhat dangerous method of construction, but produces a very solid bank.

Another remarkable dam is that of St. Christopher, upon the Marseilles canal. This was originally simply a bank of rough stone 65 feet high, for carrying the canal across the valley, and constructed with very little care, without any intention of using it as a reservoir dam. When it was determined to adapt it to this purpose the water-tight lining of the canal was removed, and the turbid water allowed to sink into the dry stonework. This produced considerable settlement, but at the same time filled up the cavities with deposit. Water was then gradually let into the reservoir. At first large quantities made their way through the dam, but the amount of leakage diminished by degrees, and the inner face of the dam was then covered with good masonry 10 feet thick at the bottom and 1½ foot at the top. This was expected to render it water-tight, but did not have this effect, and upon readmitting the water, large fissures appeared on the face, particularly near the bottom. Several rows of piles were then driven near the foot, and the cracks were filled up, and the bank is now practically water-tight.

A novel method of construction has been adopted at the Puentes reservoir in Spain. The rocks on either side of the valley are very solid limestone, but the bottom of the valley consists of sand and gravel, which rest upon a bed of marl. Arches have been thrown across the valley abutting against the rocks, and the dam, which is of masonry, is supported by them, and only the filling underneath the arches rests upon the bad foundation.

The Author points out that storage reservoirs have a very beneficial effect upon climate, as has been proved in Algeria, where the result of storing up the flood-waters in winter and using them for irrigation in summer has been the drying up

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of the marsh lands and laying them under cultivation, with a marked improvement in the health of the country.

Masonry dams frequently leak a good deal when first constructed, and this need not cause alarm, as by degrees the leakage diminishes. If, however, the ground is permeable, a serious amount of water is lost by filtration round the ends and underneath the dam. When flood-waters are stored in reservoirs, a large amount of deposit takes place, and this may be disposed of for irrigation in winter, when it serves as a manure, but in summer clear water only must be used. Special arrangements for discharging the water are necessary in such cases.

The silting up of irrigation-reservoirs is one of the most serious problems in connection with them, and it is sometimes thought that where the reservoirs are fed by mountain torrents bringing down stones and boulders, they would be filled up with these materials, but the Author's observations lead him to the conclusion this is not the case, but that detritus of this description is deposited in the beds of the streams above the reservoir, and only fine stuff is carried into it. This latter, however, amounts to a very considerable quantity. The annual amount of deposit at the Habra reservoir is 4·44 cubic feet per acre of gathering ground; at Sig 4·05 cubic feet; at Tlélat 24 cubic feet; and at Djidionia 43 cubic feet. Various methods of washing out the deposit are discussed in the Paper, which concludes with remarks upon the value of water for irrigation, and a table giving particulars and cost of twenty-one reservoirs in different parts of the world.

W. H. T.

Restoration and Enlargement of Ancient Irrigation-Works in Travancore.[1] By STEPHEN HORSLEY, M. Inst. C.E.

(Travancore Irrigation, and an account of the opening of the Pandian Canal Head-Works, 1885.)

These works lie in the southern end of Travancore, about 25 miles in length between the River Codayaur and Cape Comorin. The breadth of this tract between the watershed of the hills and the sea is also about 25 miles to the N.W., tapering to 7 miles near Cape Comorin. Of this breadth an average breadth of 10 or 12 miles along the sea is cultivated, and the rest is forest. The cultivation consists of 218 square miles of dry, and of 50 wet. Of the wet cultivation 19,071 acres draw their supply of water from artificial channels, and 12,674 acres from rainfall, which averages, perhaps, 60 inches annually. Under the channels are seven hundred and thirty-three tanks; and two thousand and fifty-six tanks are used for the wet cultivation fed by rain only. This number of tanks is very great in comparison with the area of the talooks, and this is due to the natural configuration of the ground

[1] The original is in the library of the Inst. C.E.

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rendering it impossible to make large tanks such as are seen in the flatter and more even districts of the southern portions of the Madras Presidency. The ancient works, certainly more than one thousand years old, consist of an anicut (or dam) across the Paralyaur river, and a channel cut chiefly through solid rock for a distance of two miles through the watershed, falling into the Pallyaur river at a point where its catchment-basin is only 4 square miles. Below this point, on the Pallyaur, another anicut with distributing channels converts the Pallyaur valley into a smiling valley of rich paddy land, with numerous large towns in it, virtually increasing its catchment-basin by 50 additional square miles. About one hundred and thirty years ago another anicut was constructed across the Paralyaur river, below the first, for the irrigation of its own valley, with a channel 19 miles long, in which wonderful perseverance and skill are shown by taking it through spurs and saddles in cuttings 30 to 50 feet deep, and across a valley on a bank 40 feet high, in order to economise fall to the utmost. The Paralyaur is crossed by eleven anicuts, one below the other, in a length of 20 miles, from each of which distributing channels have been led, of which the eight chief have a total length of 64 miles.

From long neglect and the natural decay of ages this system of irrigation was in a lamentable state of inefficiency. With two exceptions the channels were in a state of decay and insecurity. This neglect had recently been due to the attention of both Government and engineers being chiefly attracted away from the necessary details of placing channels and tanks in a state of efficiency to a proposal, dating many years ago, for the diversion of the Codayaur river into the Paralyaur. This went so far that in 1852 half a lac of rupees was actually assigned by the Government to commence the Codayaur dam that year; but the matter fell through. In 1869 the Codayaur scheme was again revived, in reference to the inefficient state of the irrigation; and a possibility of constructing a large reservoir on that river was considered, and plans for it made. In 1877 the late Maharajah determined to postpone construction and improvement of roads, and devote all available money to irrigation-works. Mr. Jacob submitted a report, in which, while acknowledging the necessity of increasing the storage area, he stated his belief that any scheme for that end was "premature till existing tanks and channels are put in order."

Thus, there were three schemes, viz.: (1) for a storage-reservoir sufficient for two or three weeks' supply for the whole of the irrigation; (2) for bringing the Codayaur into the Paralyaur; (3) to begin by putting existing channels into such order as to carry at least their natural supply, and to restore the tanks so that they might contain a sufficient supply without discharging to waste every season, as they did at the time. On these Major Mead, R.E., was asked to report, which he did after visiting the sites, and considering the plans and estimate of Mr. Horsley's proposals for improving the existing channels and dams. Major Mead made a

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report condemning the reservoir scheme, as not properly matured; recommending the postponement of so serious a scheme as the Codayaur proposal; and recommending the putting in hand as soon as practicable of the improvement of the channels and dams. The Travancore Government at once gave orders for the vigorous prosecution of the works recommended; and has spent on irrigation during the last five years from 12 to 24 per cent. of its total expenditure of the Department of Public Works. The views of the Maharajah are indicated in the Gazette of 11 October, 1881. The estimates were therein ordered to provide for the complete repair and restoration of the work, in each case, with reference to the area to be irrigated; and not, as at present too often the case, for partial repairs only. Many of the tanks, the Gazette goes on to say, are small ponds. They have silted up through a long course of years, and some at least will require an expenditure out of all proportion to the revenue they yield: but the numerous poor ryots holding lands under them are entirely dependent for their livelihood on the irrigation derived from those tanks, and such irrigation is now precarious owing to the state of the reservoirs. No amount of expenditure, however, will avail to improve the present condition of things, if the system is continued of going on without any method or system, repairing each tank partially, and allowing it to relapse in a short time to its former condition.

So soon as the cultivating season of 1882 was over, the work commenced: and up to last year 1,434,649 rupees had been spent almost entirely in the southern division during the reign of the late Maharajah, under Mr. Horsley. The works completed consist of:—

The Head Works, of five openings each 6 feet 4 inches × 5 feet and of a total length of 140 feet, and height of 24 feet. The screw gearing was made, including the castings, in the local workshop. These works shut out river-floods from the head channels.

The Pandian Channel, the most ancient channel in solid rock, has been widened from 12 feet to 26 feet at bottom, sloped and cleared from trees and vegetation. This secures a supply to about 17,000 acres.

The Poothen Dam, 270 feet long, 6 feet to 30 feet in height, raises the water of the Paralyaur 20 feet. The hearting of the old work has been repaired with coursed rubble in soorkee, and a face wall 10 to 15 feet high, 4 feet thick at top, built into the front of the dam with a batter of 1 in 4. This has stopped all appreciable leakage.

The Palpanabapoorum Poothenaur channel has been shortened and improved, and 900 feet of double bank 40 feet high converted into an open masonry channel.

On the 5th March, 1885, the late Maharajah, Rama Yurma, opened these works, which he had frequently visited during construction.

J. H. L.

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On the Water-Carriage and Trans-shipment of Coal.

(Centralblatt der Bauverwaltung, Dec. 2, 1885.)

The Author deals with the various methods of conveying coal by canal from the mines at Dortmund to the port of Emden, and gives full statistics of the cost of each system, comparing the same with the corresponding railway-charges.

Description of Vessels, and Arrangements for Loading and Unloading.—The coal is conveyed to the wharf at Emden:

(*a*) In ordinary barges, requiring trans-shipment of cargo,
  (1) By skips,
  (2) By elevator.
(*b*) In special vessels designed to facilitate trans-shipment, viz.:
  (3) Link-boats: (*a*) close-coupled,
    (*b*) loose-coupled.
  (4) Wagon-boats;
(*c*) In ordinary barges, with special appliances,
  (5) Trans-shipment skips.

(1) The coal is shovelled out, loaded into skips, and hoisted by cranes. Dead load of barge: gross load = 1 : 5; cargo = 460 tons. One 2-ton hydraulic crane, with gang of twelve men, clears 64 tons per hour, or 640 tons per day of ten hours. Four cranes are in use.

(2) The coal is scooped out of the barge-hold, and shot into the ship by means of Rigg's floating-elevator. Barges the same as the preceding. The elevator, with eight men, clears 56 tons per hour = 560 tons per diem. Four elevators are in use.

(3) This arrangement (introduced at Goole by W. H. Bartholomew) includes two systems:

(*a*) Close-coupled link-boats, square-framed, in series of fourteen vessels, each 25 to 40 tons. Dead load : gross load = 1 : 4·5; average cargo = 420 tons. For unloading, each vessel is floated on to the hydraulic lift at the wharf, and tipped over the ship's hold. One hydraulic lift clears between four or five boats per hour; average 35 tons each, say 157·5 tons = 1575 tons per diem. Two lifts are in use.

(*b*) Loose-coupled link-boats, with bevelled ends, in series of fifteen vessels. Dead load: gross load = 1 : 4·5; cargo = 630 tons. For navigating the river, the boats are separated into three lengths, coupled side by side, to reduce the otherwise unmanageable length of the vessel in varying currents and bends. Loading arrangements as for (*a*).

(4) The wagon-boats are of similar construction to 3*a*, but about one-fourth the size, and are close-coupled, three in a line. Dead load : gross load = 1 : 3·33; cargo = 4 tons each, No. 96 = total cargo 384 tons. These vessels are intended to be mounted on truck-frames on the railway from the mine, and lowered into the water. One 11·5-ton hydraulic crane clears twenty-five vessels

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= 100 tons per hour = 1,000 tons per diem. Two cranes are in use.

(5) The coal is loaded into the skips at the mine; they are conveyed on the railway trucks to the canal, hoisted on board, and at Emden hoisted and tipped. Dead load: gross load = 1 : 4; cargo = 368 tons. One 5·5 ton hydraulic crane clears 35 skips (average 2 tons) = 70 tons per hour = 700 tons per diem. Three cranes are in use.

Steam-tugs are used in all cases; being built in two sections, and forming, when coupled to the barges, the bow and stern respectively, thus making one self-contained vessel of the whole.

Number of journeys per annum.—The speed averages about 3 miles per hour, and with allowance for time at locks, the journey of 162 miles from the mine to the wharf occupies two days and three nights. Each boat is estimated as working forty-two weeks per annum; with varying times occupied in loading and unloading, the ordinary barges make twenty-one journeys, the link-boats nineteen, and the wagon-boats and trans-shipment skips eighteen per annum. The return journey is usually made with about one-fifth full cargo, consisting of timber and stores for the mines, and general merchandise.

Cost of the System.—(*a*) Capital outlay:

The cost of the wharf arrangements is distributed proportionately over each section (see next page).

(*b*) *Working-Expenses.*—The annual expenditure is estimated in detail as under:—

(*a*) Interest charges, 5 per cent.

(*b*) Maintenance and renewals:

	Per cent.
Tugs	7½
Skips	6
Steam-cranes	5
Elevators	4
Wagon-boats and trans-shipment skips	3½
Link-boats and inclined planes	3
Hydraulic cranes and machinery	2½
Barges, shoots, and hydraulic lifts	2

(*c*) Freight on railway from mine to canal. (At present the mines are about four miles from the canal, and a short railway conveys the coal over this length.)

(*d*) Loading: working of shoots, wagons, inclined planes, cranes, &c.

(*e*) Water-carriage:

(*α*) Wages (Tug: skipper, steersman, engineman, two stokers, cook; and two men to each barge).

(*β*) Mileage-bonus to crew = 1·93*d.* per mile.

(*γ* and *δ*) Coal and general stores for tug.

(*ε*) Lighting for night-journeys.

(*f*) Insurance of vessels and cargo: canal boats, 1½ per cent.; Rhine boats, 2 to 2½ per cent.; cargo, 2¼ per cent.; ditto on link-boats, 3 per cent.

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Number.	Item.	At £.	£.	Total £.
	(1.)			
8	Shoots	1,250	10,000	
15	Tugs (45 HP.)	1,200	18,000	
22	Barges	1,350	29,700	
4	2-ton hydraulic cranes	500	2,000	
16	Skips	10	160	
	Wharf-arrangements	..	6,250	
				66,110
	(2.)			
8	Shoots	1,250	10,000	
15	Tugs	1,200	18,000	
22	Barges	1,350	29,700	
4	Elevators	2,500	10,000	
	Wharf-arrangements	..	2,500	
				70,200
	(3a.)			
8	Shoots	1,250	10,000	
16	Tugs (in two sections)	1,325	21,200	
24	Barges (each fourteen vessels)	1,350	32,400	
2	Hydraulic lifts	3,750	7,500	
	Wharf-arrangements	..	6,250	
				77,350
	(3b).			
8	Shoots	1,250	10,000	
13	Tugs (in two sections), with special couplings	1,350	17,550	
18	Barges (each fifteen vessels)	2,100	37,800	
2	Hydraulic lifts	3,750	7,500	
	Wharf-arrangements	..	6,250	
				79,100
	(4).			
8	Inclined planes	800	6,400	
17	Tugs (in two sections), with special couplings	1,350	22,950	
31	Barges (each ninety-six sections)	1,650	51,150	
2	Hydraulic cranes (11·5-ton)	650	1,300	
	Wharf-arrangements	..	6,250	
				88,050
	(5.)			
8	5½-ton steam cranes	500	4,000	
18	Tugs	1,200	21,600	
28	Barges	1,350	37,800	
32	Sets skips	1,200	38,400	
3	5½-ton hydraulic cranes	525	1,575	
	Wharf-arrangements	..	6,250	
				109,625
	Total			490,435

(*g*) Tolls, harbour-dues, &c., 6*d.* per ton; canal dues, 1*s.* 5½*d.* per ton.

(*h*) Trans-shipment:

	Per Ton.		Per Ton.
	d.		*d.*
1.	3·97	4.	3·1
2.	4·14	5.	3·0
3*a* & 3*b*	3·3		

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(*i* & *j*) Administration, agents, and miscellaneous charges.

Freight-Charges.—The total of all the foregoing items enables the coal to be conveyed at the following charges:

		s.	d.
1.	Loaded by skips	3	6
2.	„ „ elevator	3	7
3*a*.	Link-boat, close-coupled	3	9¾
3*b*.	„ loose „	3	8
4.	Wagon-boat	4	2½
5.	Trans-shipment skip	4	4¾

As against this, the cost of conveyance by rail is as follows:

		s.	d.
Carriage to Emden	per ton	4	2½
Use of sidings		0	1¾
Loading and storing		0	3
General charges		0	1½
		4	8¾

P. W. B.

The Rothesand Lighthouse at the Estuary of the Weser.

(Centralblatt der Bauverwaltung, Jan. 2, 9, 16, 1886.)

The Rothesand forms a shoal dividing the Estuary of the Weser into two channels. At the spot where the lighthouse is constructed there is a depth of about 26 feet of water at low tide, and it is often exposed to very heavy seas. The first foundation was attempted in 1881, but was swept away in October of the same year. The caisson for the new structure was floated into place in May, 1883, when the work of levelling the bed and of protecting the site by a breastwork of fascines and rubble was actively proceeded with. The workmen lived on board two vessels moored close to the work; but great difficulty was experienced in getting materials to the tower, as this was often attended with some danger, though facilitated later on, when the two steam-cranes were got into operation. The caisson formed two segments of a circle of 23 feet 6 inches radius, the length being 46 feet and the breadth 36 feet. The total height over the crane-posts was 89 feet, and to the machinery floor 42 feet 6 inches; the working-chamber at the base of the caisson was 8 feet 2½ inches high, and the central shaft 3 feet 3 inches diameter.

The lower part of the structure is formed entirely of concrete; towards water-level the outer casing is brick in hydraulic cement. By the middle of October, 1883, the foundation had been sunk to 51 feet below low-water; and at this stage the caisson was considerably damaged by a severe storm, and work was suspended till the following February (1884), when the foundation reached the specified depth of 72 feet below low-water, and the machinery was

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removed. The sinking was effected entirely by exhaust air through six 1½-inch pipes (which were left embedded in the concrete as the work proceeded), the foundation being on hard fine sand with small shells and flaky boulders, overlying a bed of coarse shingle.

The completion of the foundation occupied 650 hours, in which about 1,700 cubic yards of material were removed. The time-record to the end of the first year (June 1, 1884) was:—187 days on which work was possible = 50 per cent. of the whole time; on these, 2·22 working hours = 25·15 per cent. of the whole time. The best month was April, 1884, when there were 23 working days.

After the completion of the foundation, the construction of the tower was rapidly proceeded with, and by November the superstructure was complete to 65 feet above low-water. Soon after this, work was suspended for the winter, having from the commencement up to this stage occupied 285 days (= 54·29 per cent. of time) = 3,450 hours or nett 27·38 per cent. of the total time.

The work was resumed in April, 1885, when, in addition to the ironwork, the interior fittings were proceeded with. Considerable care and technical skill were required in the completion of the breastwork of fascines and rubble, carried to a distance of 50 feet all round the tower, and in which over 7,000 cubic yards were thrown down and secured: the depth of water, when this work was finished, varying from 22 feet at the tower to 27 feet at the toe. The breastwork was completed in August, at the same time as the ironwork of the tower; and the lighting apparatus was then erected by Veitmeyer, of Berlin.

The whole superstructure is of iron, painted in broad alternate bands (about 13 feet wide) of black, white, red, and white; which renders the lighthouse a most conspicuous and easily recognised object. The concrete and brick base of the tower, above low-water mark, is also cased with wrought-iron plates.

The first cable message was sent from the lighthouse at the end of September, and on October 23rd the work was completed. The lights were exhibited for the first time on November 1, 1885.

The tower comprises four chambers:—a fire-proof entrance-room or cellar, with cisterns sunk in the concrete, store-room, kitchen, and living-room. Above this, the head of the tower forms the central lamp and service-room, from which project three turrets. In two of these are auxiliary lights for the intermediate bends of the channel, while the third contains a staircase leading to the central lighting apparatus, as well as a third light. These turrets give a further distinctly characteristic appearance to the tower.

As the above apparatus exhibits eight distinct lights to serve the navigation of the intricate surrounding channels, reference to the chart is necessary in order to understand their precise arrangement. The head-light is of the 4th order, and is 88 feet 3 inches above low-water; the 5th order light in the third turret is 85 feet 4 inches, and the two secondary turret-lights of the

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6th order (for the navigation of the bend past the lighthouse) are 72 feet above low-water.

The head-light shows both sea and riverwards a fixed light for the deep-water channel, two flashes for the easterly approaches to this channel, and one flash for the western side. The 5th order apparatus shows a fixed light towards the Elbe Estuary and Heligoland.

The contract was let at £42,650 to the Harkort Company, of Duisburg, by whom the work was most satisfactorily carried out. Small alterations made during the progress of the work brought up the total cost to £43,400.

P. W. B.

Railways of Europe in 1884.

(Revue Générale des Chemins de fer, October 1885, p. 259.)

The total length of railways opened for traffic at the end of the year 1883, amounted to 113,716 miles. At the end of 1884 the length open was 117,653 miles, showing an increase of 3,937 miles opened in the course of 1884, or 3·46 per cent. of the total length at the end of 1883. The following Table shows the length of railways open, in each European State, at the end of 1884:—

RAILWAYS OF EUROPE, 1884.

Designation of State.	Length of Railways open for Traffic at the end of 1884.	Increase per cent. in 1884, above the length open at the end of 1883.
	Miles.	Per cent.
1. Germany	22,830	2·31
2. Austria-Hungary	13,736	5·99
3. Belgium	2,684	1·08
4. Denmark	1,208	7·23
5. Spain	5,383	4·99
6. France	19,397	5·05
7. Great Britain and Ireland	18,962	1·11
8. Greece	109	695·45
9. Italy	6,167	4·97
10. Low Countries and Luxemburg	1,649	5·28
11. Portugal	949	2·21
12. Roumania	995	5·39
13. Russia and Finland	15,778	2·02
14. Servia	152	..
15. Sweden and Norway	5,072	2·54
16. Switzerland	1,716	0·40
17. Turkey, Bulgaria, and Roumelia	866	..
Total	117,653	3·46

Of the total increase of length of railway opened in the course of 1884—namely, 3,937 miles—515 miles were opened in Germany,

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776 miles in Austro-Hungary, 256 miles in Spain, 933 miles in France, 208 miles in Great Britain and Ireland, 292 miles in Italy, 313 miles in Russia and Finland, 152 miles in Servia, and 125 miles in Sweden and Norway.

D. K. C.

The Harz-Railway, Blankenburg to Tanne (combined Rack-rail and ordinary Railway). By E. PETERS.

(Zeitschrift für Bauwesen, 1886, p. 71.)

This line was commenced in August 1884, and opened from Blankenburg, as far as Rübeland, a length of 5·9 miles (9½ kilometres) in November 1885.

It is expected to be completed to Tanne this summer, with an ultimate continuation southwards, to form a junction with the existing Scharzfeld, Lauterberg and St. Andreas Railway.

The mountainous character of the country to be traversed, necessitated the adoption of very sharp curves and steep gradients, and the nature of the traffic, most of which is mineral (the ironstone district of Braunesumpf near Hüttenrode being intersected), made it desirable that the gauge should be that of the main lines, viz., 4 feet 8½ inches, and the railway throughout is laid to this gauge, and worked in the ordinary manner excepting where the gradients are steeper than 1 in 40. On portions of the line, steeper inclines were desirable, from an economical point of view, and it was decided to work these on the central rack-rail system, that adopted in this instance being known as "Abt's."

The railway commences at Blankenburg, its chief station, by a junction with the Halberstadt Railway, and the following is a Table of the various stations, distances and levels, showing the character of the country, compiled from the section accompanying the original Paper:—

Distance.		Station.	Ordnance Level.
Miles.	Kilometres.		Feet.
0	(0)	Blankenburg	755·1
1·4	(2¼)	Bast	1,059·4
3·7	(6)	Hüttenrode	1,565·9
5·9	(9½)	Rübeland	1,240·5
8·3	(13½)	Elbingerode	1,449·7
12·0	(19½)	Rothehütte	1,413·7
16·3	(26¼)	Tanne	1,509·5

Bast is a reversing station, and reckoning the line there as a double distance, the total length of the railway is increased to 16·9 miles.

Of this length 48 per cent. is on the straight, and the remainder on curves varying from 9 to 50 chains radius, but as a rule the radius is not less than 15 chains. As regards gradients, 3·3 miles or 19 per cent. is level, and 13·5 or 81 per cent. on gradients. Of

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the latter, 4·1 miles are on the rack-rail inclines, of which there are ten with gradients varying from 1 in 16·67 to 1 in 28·6—3 miles, or ⅘ths, being on the steepest grade, viz. 1 in 16·67. As before stated, the portion worked in the ordinary manner, or by adhesion, is nowhere steeper than 1 in 40, the mean being 1 in 44.

The width of formation on embankment varies from 13 feet 6 inches to 15 feet 5 inches according to the height, and in cutting, the width is 12 feet 1½ inch. There are two tunnels, 514 and 202 yards long, and a viaduct of four 26 feet spans.

The permanent way is illustrated by numerous diagrams. The rails on the ordinary adhesion track are of steel, 4$\frac{11}{16}$ inches deep, 2¼ inches breadth of head, and weighing 60½ lbs. per yard. The cross-sleepers are of mild steel 7 feet 3 inches long, 8½ inches broad, 2½ inches deep, weighing 88 lbs. each. To the concave underside of these are riveted two cross-pieces of angle-iron, 2 feet 6 inches apart, equidistant from the centre line, to give the road lateral resistance, on the ballast; they are slightly bent upwards at each end to give the proper cant to the rails, the latter being attached to them by clamps and keys. The sleepers are placed an average of 3 feet apart, centre to centre, on the adhesion portion, and 2 feet 10½ inches apart on the centre rack-rail portion.

The rack-rail is triple, and made up of three toothed steel-bars placed 1⅜ inch apart with its surface raised 2⅜ inches above the ordinary rails. Each bar is 4¼ inches deep, ¾ inch thick, and 8 feet 8 inches long, or three times the distance apart of each sleeper. They are laid to break joint, one with another, so that over the centre of each sleeper there occurs a joint in one of the three bars. They are kept vertical and supported at each sleeper by being bolted to a cast-iron pedestal keyed down to the centre of the sleeper.

A description is given of the arrangement at the ends of each length of line laid with the centre rack-rail. This consists of a length of triple rack-rail, hinged at its upper end to the fixed rack-rail above described, and is tapered off at the lower end. It is supported longitudinally at four points by vertical helical springs butting against plates bolted down to a concrete foundation.

The locomotives have each two pairs of cylinders, the one pair for the ordinary driving-wheels, and the other for actuating the toothed wheels (of which there are two) for the rack-rail.

The diameter of the pitched circle of each toothed wheel is 1 foot 9½ inches. The weight of the engine ready for running is 54 tons.

D. G.

Horse-power in Propelling Tram-cars. By A. W. Wright.

(Journal of the Association of Engineering Societies, Jan. 1886, p. 88.)

On the tramways of North Chicago City, the cars travel at a speed of 6 miles per hour. The average service-life of the horses is five years, travelling on selected cobble-stone pavement. Before the cobble-stone was laid the average life was four years. On the

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principal tramways of the United States, the length of the service-life is estimated at from three to five years. On old worn-out iron rails in the North Chicago Railway, the resistance of a car, weighing, with an average of 14·8 passengers, 6,772 lbs., averaged 109·5 lbs., or 32·3 lbs. per ton, at a speed of 5 miles per hour, including stoppages. On new steel rails, the resistance was only 59·8 lbs., or 15·6 lbs. per ton. The respective forces required to start from a state of rest were 134·6 lbs., and 116·5 lbs. per ton. It is calculated that the average power exerted by a team of horses is 1·208 HP., or 0·604 HP. for each horse.

The greatest exertion of force occurred in starting a loaded car. It was at the rate of 283·5 lbs. per ton.

D. K. C.

Metal Sleepers.[1]

(Paper presented to the Brussels Railway Conference, 1885.)

The strength of an iron sleeper should be much greater than that of a timber one, because the former must offer sufficient resistance to deflection over its whole surface, however well it may be packed, as the reactions of the ballast, during the passage of trains, are distributed uniformly over the whole of the packed portion. Metal sleepers do not bend visibly, like those of wood, but descend evenly; and the leverage of the moment of deflection at the intersection of the rails and sleepers is greater than in the case of timber. But, if the strain be limited to 6 kilograms per square millimetre (nearly 4 tons per square inch) for iron, and about half as much again for steel, there is no occasion to fear for the resistance of metal sleepers. If the ballast inside the rails has not been properly packed, so that only the portions outside the rails sustain the whole load, the moment of deflection will double; and even then there will be nothing to fear. But if, on the contrary, only the central portion of the sleeper between the rails bears upon the ballast, the maximum moment of deflection, which may be five times greater, will be transferred to the centre of the sleeper. Experience has, however, shown that this will not occur if care be taken not to pack the central portion of the ballast.

The Vautherin sleepers, tried on the Belgian State Railways in 1868, when there was only a weight of 6 tons on each wheel of the locomotive, would give, on the passage of a train—supposing the sleepers properly packed for 50 centimetres (20 inches) on either side of each rail, so as to distribute the reactions uniformly outside the rails—a moment of deflection equal to $0{\cdot}45 \times 6000 \times 225 = 607{,}500$ kilograms, or $607\frac{1}{2}$ metric tons.

Now $$I = 1{\cdot}015953 \frac{I}{V} = 29881.$$

[1] The original is in the Library.

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Therefore R = 20·4 kilograms per square millimetre, or nearly 13 tons per square inch, I being the moment of inertia, V the distance of the most strained fibre from the centre of deflection, and R the strain. All these sleepers split or broke at their intersection with the rails, the fractures being caused by the great strain on the metal when subjected to deflection. Filling hollow sleepers with ballast having a certain amount of cohesion is not recommended, as no degree of elasticity in the filling substance can increase the resistance to deflection.

The subject of fastenings is of greater importance as regards the prevention of shocks between the rails and sleepers than in preserving the gauge. If the distance between the rails laid on metal sleepers should increase, that is due to their having been originally of insufficient strength; so that, after having been bent for canting the rail, they have eventually become straightened under the load they had to sustain. Whatever be the method adopted for attaching the rails to the sleepers, it must be better than any hitherto employed for wood sleepers; and cotters are decidedly preferable to bolts, the threads of which rust up so that the nuts cannot be tightened. The amount of bearing surface of the sleeper on the ballast, provided it be of good quality and incompressible, is of less importance than that the pressure exerted by the ballast on the underlying soil, which is often compressible, be distributed over the largest surface possible. It is not necessary that the sleepers have a large bearing surface on the ballast, nor that they be flat on their underside, but only that there be a sufficient width between the outer edges.

The Author proposes to retain the cast-iron chair, which has rendered good service, especially with Vignoles rails, and to connect each pair of chairs by two bars of Z section, capable of standing a deflection of 6 kilograms per square millimetre (nearly 4 tons per square inch) keyed to them so as to form a trough turned either upwards or downwards. The following Table gives the weight and cost, including fastenings, the moment of inertia, ratio of the latter to the moment of resistance at the centre of deflection I : V, the moment of deflection M, and resistance R, in kilograms per square millimetre with corresponding tons per square inch, of the proposed sleeper compared with those tried on the Belgian State Railways:—

Type.	Weight.	Cost.	I.	I : V.	M.	R.	
						Per Square Millimetre.	Per Sq. Inch.
	Kilog.	Fr.	Kg. per Sq. MM.	Kilog.	Kilog.	Kilog.	T.
Vautherin . .	35·0	6·0	1,015,953	29,881	607.300	20·4	13·0
Post . . .	79·5	10·50	2,144,042	37,000	777.924	21·0	13·3
Braet . . .	79·5	10·40	3,624,768	50,840	777.924	15·3	9·6
Bernard . .	113·0	18·20	9,034,642	98,209	878.047	8·95	5·7
Iron Z bars .	89·0	10·0	6,646,032	120,837	729.316	6·0	3·8

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From which it appears that the iron-trough sleepers are both the cheapest and the strongest. Instead of Z bars, light iron joists, such as are used for the frames of railway wagons, may be employed; or, better still, old steel rails of Vignoles section, cut to lengths of 2·4 metres (7 feet 10 inches), each pair, with the chairs between, being connected by rivets, thus forming a cheap, strong, and durable sleeper, which will be worth nearly as much as new sleepers after they have both served their turn, while at the same time using up what is now a drug in the market.

Although steel is preferable to iron for surfaces liable to wear, like those of rails and tires, it presents drawbacks in the case of parts subject to deflection only, such as bridges and sleepers, besides being brittle in winter, and requiring special precautions while cooling on leaving the rolls.

J. W. P.

Photometric Experiments on Gas and Gas-Burners carried out at the Physical Laboratory of the Royal Technical Institute of Turin.

By S. PAGLIANI, Professor at the Institute.

(L'Ingegneria Civile, 1885, p. 90.)

These experiments refer to a method of burning gas invented by Messrs. Monari Celestino, Sardè Augusto, Vinci Francesco, and Baldi Antonio, of Bologna, called the *lucentina solare*, which consists in increasing the illuminating power of the gas by adding vapour of solid hydro-carbons, the system being an improvement of that of Messrs. Kidd and Livesey, of London. The mixture adopted is 8 of naphthalene to 1 of kerosene. The inventors claim that this effects a saving of from 40 to 50 per cent. The burners to be used are Bray's No. 0, 1 and 2. The mixture is heated either by the flame of the burner, or by a separate flame. Kerosene contains about 85, naphthalene about 93, per cent. of carbon.

In 1884 a public trial of the gas was made by lighting one of the principal squares in Turin with it, and the Municipality requested the writer and Mr. Antonio Burzio to carry out photometric experiments. The methods adopted are described in the Paper, and they showed that the mixed or carburetted gas, as compared with the ordinary gas, has an illuminating power of from 5·6 to 1 to 7·5 to 1, the mean being 6 to 1. The economical results obtained from the experiments, and from a lengthened trial in public lighting, show that with the burner Bray No. 1, the carburetted gas costs per hour and per Carcel-unit two-thirds more than ordinary gas. With burner No. 2 one-twentieth more; and with burner No. 3 one-third less. That is to say, that with the same burner, the same expenditure gives an illuminating power one-third more than when burnt in the usual way. The effect of this may be, however, to produce too strong a contrast of light and shade if the lamps are kept at the same distance apart, and it may

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be found necessary to place them nearer together, in which case it is doubtful whether any real economy would be effected.

Experiments were then made upon ordinary gas burnt in the Ravanelli burner.[1] The special feature of this invention is that the gas on its way to the burner is heated in a small chamber over the flame. These experiments showed a saving of from 17½ to 21½ per cent. due to using this system.

W. H. T.

On Water-Gas. By E. Blass.

(Stahl und Eisen, vol. vi., 1886, p. 3.)

In this Paper, which was read before the German Iron Smelters Association at Dusseldorf in December 1885, the Author, after noticing the methods adopted by Lowe, Tessié du Motay, and Strong for the production of artificially carburetted water-gas for illuminating purposes, proceeds to describe several modifications of Strong's apparatus made and used by Messrs. Schulz, Knaudt and Company, of Essen, which are essentially adapted for the manufacture of heating-gas. The first of these, which has a working capacity of 10,000 cubic feet per hour, consists of an iron jacketed cylindrical producer whose interior section is like that of a foundry cupola, and diminishes in diameter from about 50 inches at the top to 39 inches at the level of entry of the blast, about 8 feet down, where the most active combustion takes place. The fire-brick lining is protected at the latter point by a ring-shaped trough in which water is kept flowing. Below the twyer-level the stack widens out, forming a receptacle for the slag and clinker of the fuel. This lies in a conical talus, leaving an annular space round it, through which the blast and gas are alternately admitted and exhausted. An empty space of about 3 feet deep is left at the top of the stack for the accumulation of gas, which communicates by a passage on one side with the regenerators, and provision is made for the introduction of air at the top by a branch from the main blast-pipe. The regenerators are two cylindrical towers; the first is divided by vertical walls like a Whitwell stove, while the second is filled with the ordinary Siemens checker brick-work. The fuel, which is either small coke or the breeze and cinders from the ashpits of smithy- and heating-furnaces, is charged through a funnel, covered with a gas-tight lid and stopped with a conical plug, attached to a rod which slides through the cover. When the producer is filled with fuel and lighted, air is blown in at the bottom and the gas produced (carbonic oxide and nitrogen), rising through the fuel, is fired in the upper gas space by the top blast, and the intensely heated products of combustion are cooled down by passing them through the two regenerators. At short intervals the blast is stopped and a current of steam, superheated

[1] Minutes of Proceedings Inst. C.E., vol. lxxx., p. 438.

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by passing through the regenerators, is sent through the heated coke in the reverse direction, producing water-gas (carbonic oxide and hydrogen), which is led through the port at the bottom to a scrubber filled with coke, where it is cooled and purified from dust, and finally passes to the gasometer. The method of working given above has since been modified by abandoning the combustion of the producer gas with the top blast, as, on account of the very ashy character of the fuel, so much finely-divided flue-dust was carried over by the heated gases that the brickwork of the regenerators suffered considerably from slagging. The gases are therefore allowed to pass off at their natural temperature, and the steam is consequently superheated to a lower point than previously, about 500° instead of 800°, but the production of water-gas has been doubled, while the consumption of fuel has not apparently increased. The working periods are fixed at ten minutes for blast, and five minutes for steam-admission, the blast, gas, chimney, and steam-admission valves, being all connected by gearing in such a way that they may be opened and shut in the regular order by turning a single hand-wheel to the right or left. The passage, which serves both for the admission of the blast and exit of the gas, is governed by a slide-valve which, together with its seat, is cooled with water. When the fuel is hot and the steam newly admitted, the gas produced is practically free from carbonic acid and steam, but as the producer cools both gases increase in quantity. The amount of water condensed from 1,100 litres of the gas was 24 cubic centimetres, or about 4 per cent. The average composition of the gas as a whole is nitrogen 5, carbonic acid 4, carbonic oxide 41, and hydrogen 50 per cent. This is produced at the rate of 250 to 300 cubic metres per hour, with a consumption of 1·2 kilogram of coke per cubic metre. A point of considerable interest is the presence of silicuretted hydrogen in the first portion of the gas produced when the steam is admitted. This, although not in weighable quantity, is sufficient to colour the flame strongly orange yellow, and as the amount of carbonic acid increases it is decomposed into silica and water, the former substance forming a fine cloudy deposit in the scrubber and gas-holder.

A smaller apparatus intended to produce 50 cubic metres (1765 cubic feet) per hour, described by the Author, is of very much simpler construction, having neither blast nor regenerators. The place of the former is supplied by a Körting aspirator worked by a steam-jet; the same aperture at the top serves as a chimney and a charging passage. The aspirator, charging-funnel, and stop-valve, are worked on a horizontal plate, which turns upon a fixed pivot, so that either one of them may be brought into connection with the interior of the producer when required, and the pivot is bored as a two-way cock for the alternate admission of steam into the chimney and the producers, the whole being connected together by gearing wheels in the same way as in the larger apparatus. This consumes per cubic metre of gas made 1·5 kilogram of coke with 14¾ per cent. of ash, and 5 per cent. of water, and 1 litre of

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water as steam equal to 0·15 kilogram of fuel. The cooling-ring uses 3·5 cubic metres of water in 24 hours. The scrubber requires 170 litres of water per hour.

A third form of apparatus of the capacity of 1,000 cubic metres per hour, but actually producing about one-half that quantity, is in use at Essen. This has two producers and two scrubbers, one for producer- and the other for water-gas, which are worked together, to furnish mixed gas both for lighting and heating purposes. When these are mixed in the proportion of one of the latter to four of the former, and supposing the water-gas to contain 1 kilogram of carbon per cubic metre, the average composition of the resulting 5 cubic metres will be carbonic oxide 37, hydrogen 10, nitrogen 53 per cent., which on combustion will develop 7,000 units, or about $\frac{1}{4}$ of the total theoretical heating power of the fuel.

About 5 cubic metres of this mixed gas, or 2 cubic metres of water-gas, are required per HP. per hour to work a gas-engine. The slags from the fuel being cooled by the water-ring solidify, and form a casing round the talus of fuel in the bottom of the producer, which is removed through four passages with Morton's doors round the bottom every six hours. The cleansing requires from fifteen to thirty minutes, according to the character of the fuel.

The three sizes of apparatus are all in use. The original one was worked for three years, and is still gas-tight in all the joints and passages. The stack is somewhat burnt out above the water-ring, but is otherwise undamaged. The first regenerator has been renewed with Whitwell walls instead of the small brick chequer work.

The American plan of carburizing with petroleum residues, being unsuited for use in Europe as too costly, Fahnejelm's magnesia glow-light has been adopted to render the gas useful for illuminating purposes. This consists of a comb or rod of caustic magnesia which is raised to a white heat by a jet of the burning gas. This produces a whiter light than the electric glow-lamp, and is perfectly steady. The consumption is about the same as that of ordinary coal-gas for an equal amount of light produced.

With 150 litres per hour one comb costing about 2*d.* gives twenty to twenty-two candles at first, fifteen candles after eighty hours, and ten candles after one hundred hours' use.

The cost of the different sizes of apparatus is as follows .—

	£.
1. For 100 cubic metres per hour, with two producers, scrubber, purifier, regulator, gas-meter, and gasometer of 200 cubic metres, with steam-engine, pipes, and buildings about	1,350
2. For 500 cubic metres, including two producers, two superheaters, scrubber, purifier, regulator, gas-meter, blowing range, gasholder of 360 cubic metres, pipes, and buildings	3,000
3. For 1,000 cubic metres, with two scrubbers, and gas-holder of 1,000 cubic metres capacity	3,500

The cost of production of water-gas may be estimated as varying between 4*d.* and 1*s.* 3*d.* per 1000 cubic feet, according to the size of

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the apparatus and the price of fuel. The consumption of the latter may be from 1 to 1·5 kilogram per cubic metre. The blowing power required is $\frac{1}{50}$ HP. per cubic metre per hour. About six hundred water-gas lights are in use on the patentees' works, who also use the gas in a furnace for welding corrugated boiler-flues.

In heating rooms with water-gas a consumption of $\frac{1}{100}$ cubic metre per hour per cubic metre of enclosed space is required to raise the temperature 20° above that of the external air.

The following general data concerning water-gas are given in conclusion :—

1 cubic metre of water-gas contains theoretically—
0·5 cubic metre CO + 0·5 cubic metre H
= 0·27 kilog. C + 0·0488 kilog. H + 0·358 kilog. O
= 0·625 kilog. CO + 0·0488 kilog. H.

1 cubic metre of water-gas requires for its production—
0·403 kilog. steam + 0·27 kilog. carbon
= 0·68 cubic metre steam 0·27 kilog. carbon.

1 cubic metre of water-gas requires for combustion—
2·387 cubic metres or 3·1 kilog. of air, and develops 3,023 heat-units and a maximum temperature of 2,800° Centigrade.

1 cubic metre of water-gas requires for its production 775 units.

1 kilog. carbon converted into mixed gases gives—

1 cubic metre water-gas + 4 cubic metres producer-gas.

CO	1·36 0·50	cubic metre	= 2·325 kilog.	
H	0·50	„	= 0·0448	„
N	2·65	„	= 3·210	„
	5·00	„	5·5798	„

which, when burnt, develop 7,000 units, and produce a temperature of 2,200° Centigrade.[1]

1 cubic metre of producer-gas, with 30 per cent. of carbonic oxide, requires 1 cubic metre of air for combustion, and develops about 1,000 units and a temperature of 1,890°.

1 cubic metre of mixed gases (CO 37, H 10, N 53 per cent.) requires for combustion 1·27 cubic metre of air, and develops 1,400 units and a temperature of 2,200° Centigrade.

In the discussion on the Paper, Mr. Lürman remarked that probably a mixed gas of the kind described by the Author might be more cheaply manufactured by using a producer similar to an ordinary blast furnace, blowing in air and steam from the bottom, the amount of the latter to be so proportioned as to deliver the gas at the top of the temperature of about 100°. Preferably the steam might be admitted at a somewhat higher level than the blast. The Author in reply stated that the admission of steam from the bottom

[1] Or 1,000 cubic feet weigh 69·72 lbs.

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had been tried, but the cooling effect was so great that the slags were chilled and could not be removed, so that the plan of downward steam-circulation was resumed. In a small experiment-furnace, about a foot square, heated by water-gas, bar-iron is brought to a welding heat in three or four minutes, and soft basic steel has been melted in a crucible with cold gas and cold blast.

H. B.

The Working of Water-Gas Furnaces. By Otto Knaudt.

(Gesundheits-Ingenieur, 1885, p. 603.)

It was not until after the middle of the present century that any attempt was made to employ solid fuel for industrial purposes on other than fixed hearths or fire-grates, whereas liquid substances, or oils, had been used for illuminating purposes for thousands of years, and gas had been employed in a similar way since the beginning of the century. The first experiments in the practical use of gaseous fuel were those of Siemens, which have already led to the extended application of regenerative furnaces for manufacturing operations, while the gas-producers of Bicheroux, Dowson, Lürman, and others differ mainly from those of Siemens in that they necessitate the proximity of the generative apparatus to the place where the gas is consumed. All of these generators produce a gas of similar composition, the normal formula being 70 per cent. of nitrogen to 30 per cent. of carbonic oxide, the latter gas varying in volume as much as 10 per cent., in accordance with the quality of the coal employed. The reaction of steam upon glowing carbon, which at high temperatures may be stated as $C + H_2O = CO + 2H$, and at lower temperatures as $C + 2H_2O = CO_2 + 4H$, had been known as a laboratory experiment for many years; the credit of having turned this scientific fact to industrial account is due to the American inventors, Messrs. Lowe and Strong. By means of their process a gas differing entirely from that of Siemens is obtained. Using anthracite, Lowe succeeded in producing in America a gas which consisted of 50 per cent. of carbonic oxide, and 50 per cent. of hydrogen, and by subsequently adding to the fire-chamber a regenerating chamber, good commercial results were obtained. The successes of Mr. Dwight, in England and elsewhere, were however less marked, but after many experiments, the firm of Schulz, Knaudt and Co., of Essen, were able to use this gaseous fuel for manufacturing purposes.[1] Their first attempts were seriously interfered with by explosions of the gas; one of which, on a day when some gentlemen interested in the invention had assembled to witness some practical trials, blew several hundredweights of molten iron into the air, and led to a speedy termination of the experiment. The apparatus was then entirely remodelled by Mr. Blass and Dr. Bunte; the modifications introduced con-

[1] Minutes of Proceedings Inst. C.E., vol. lxxxi., p. 385.

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sisted mainly in the sealing up of all the apertures at which air could be drawn in; in doing away with the apparatus for the recovery of the ammonia, which is now used simply for cooling the gas, and in the provision of a hopper for feeding the furnace, by which the outburst of gases was avoided. In the furnace as now altered, the process is carried on with complete success, and a new apparatus of much larger size has been put in hand. As now worked, the regenerating chamber can be heated in ten minutes, and in five minutes 60 cubic metres of gas are produced. A complete round thus requires a quarter of an hour, and 240 cubic metres of gas are obtained hourly. Washed cinders, having 80 per cent. of ash, are used in the furnace, and each kilogram of fuel yields 1 cubic metre of gas. The gas is chiefly used for the manufacture of wrought tubing.

In a smithy fire using 20 cubic metres of gas per hour, and with a forced blast from the blower, it becomes possible to raise a bar 33 millimetres in diameter to welding heat in three minutes, with a minimum waste of iron. For lighting purposes this gas, in conjunction with a glow-lamp composed of magnesia filaments, devised by Mr. O. Fahnejelm, gives excellent results.

G. R. R.

The Thickening of Gas-Tar in Hydraulic Mains.

A Paper read at the meeting of the Baltic Society of Gas- and Water-Engineers, Dantzic, by — Von Kunath.

(Journal für Gasbeleuchtung, 1885, p. 910.)

In a former communication on the above subject, the Author considered that the causes of thick tar in the hydraulic main were twofold, mechanical and chemical, and gave his views as to the prevention of mechanical thickening through coal-dust being carried forward with the gas. The present communication deals with and explains further experiments made upon the chemical cause. A miniature gas-works was employed, consisting of a Ω shaped fire-clay retort with fittings, an air and water condenser, scrubber and purifier, and a gas-holder of 23 cubic feet capacity, the holder being counterbalanced so as to admit of its being used as an exhauster; glass was also inserted in the sides of the hydraulic main, so that the conditions of the crude gas in it could be observed, and one hundred and sixty-seven experiments were made with this apparatus, from which the following results are given: 1. High illuminating power and large yields of gas are incompatible, except so far as may be due to favourable working conditions and good qualities of coal. 2. The illuminating power of gas from small coal is considerably lower than from large coal of similar quality, which is due to changes in its chemical combinations from storing and exposure, and not to an alteration in the physical nature of the coal by mechanical breaking. 3. Dividing coals, according to their appearance, into

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bright coal, dull coal, and shale; the bright coal yields the most and the dull coal the best gas. Slaty coal gives little and bad gas. 4. Only the bright coals yield good coke. 5. Each description of coal yields a certain quantity of gas at a given temperature, the quantity being increased by increased temperature; and coke obtained at a certain temperature yields more gas if the temperature is increased. 6. Finely divided carbon may be washed out of the tar by turpentine or benzene, the quantity varying with the consistency of the tar; this carbon imparts the colour to the tar, and it may be raised to a red heat without sensibly altering its structure or volume.

The complete thickening of the tar in the hydraulic main, which sometimes occurs in a few hours, is caused by the collection of this carbon or soot, like a floating cover, on the top of the sealing liquid in the main, where it sometimes fills the entire gas-space and then suddenly sinks into the liquid, forming with it a dense tough mass. The only indications of this taking place are dryness and stoppages in the ascension-pipes, and a decrease in the illuminating power of the gas; but in order to obtain timely warning of its occurrence, it is recommended that the temperature in the ascension-pipes should be observed with a thermometer; and, if the ascension-pipes of retorts in action are so observed, it will be found that the maximum temperature in the pipes, producing tar that will flow, and giving practically no difficulty in working, is considerably higher than in those that tend to thicken the tar. Under ordinary conditions this temperature is about 122° Fahrenheit before the charge is put in; it then rises in the first five to ten minutes to the maximum of about 482° Fahrenheit, and then falls fairly uniformly back to 122°, at which it remains; but, if the maximum temperature is not reached, the formation of thick tar takes place. The temperature of the ascension-pipes and thickening of the tar were found to stand in inverse relation to the weight of the charge, and, by varying this, it was found possible to produce or avoid the tar thickening with temperatures in the retorts varying from dull red to the brightest orange.

The Author summarises the results of his latest experiments as follows:—

1. The tar-thickening commences in the retort by the separation of the solid carbon in the form of soot, and breaking up the tar-oil vapours through over-heating. If this over-heating is reduced by the introduction of steam into the retort, or by covering the charge with a non-conducting material, such as packing-paper soaked in salt water, the formation of soot is diminished so long as the means employed prevent radiation of the heat.

2. The soot, once separated, is equally uninfluenced by condensation or solvents, and no cooling or special arrangements in the retort-mouthpiece, pipes, or hydraulic main will prevent the formation of thick tar.

3. The tendency of the soot to separate occurs with all description of coal, and is the combined result of the temperature, area of retort and weight of charge, and stands in inverse relation to the

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speed at which the gas travels in the retort. The rate of separation varies, however, with different sorts of coal, being in general less in quantity with quick carbonizing coal than with coal that carbonizes slowly.

4. A loss of illuminating power in the gas always occurs from the separation of the soot, and the separation is always greatest during the first quarter of the time of carbonization.

To minimize these evils it is recommended that the gas-space in the retort should be reduced, and the surface of contact lessened as much as possible, thereby moderating the development of the crude gas and increasing the speed with which it passes off, so that it has less time to become over-heated.

C. G.

Manufacture of Ammonia from Urine in Turin.

By Professor SOBRERO ASCANIO.

(L'Ingegneria Civile, 1886, p. 12.)

In an article on the sewerage of Turin, the Author states that in a town in which there are a number of urinals, the manufacture of ammonia can be carried on with advantage, and that in Turin itself the contents of about four hundred are thus disposed of by a contractor. At each urinal a water-tight tank has been constructed into which it drains; the tank is closed hermetically by a stopper, which is removed for the abstraction of the liquid by a pump attached to a barrel-cart, in which it is conveyed to the works. The urinals are washed out daily, and there is no offensive odour from them, nor is any occasioned by the operation of emptying them. About 1,760 gallons of urine are obtained per day. Were the whole seven hundred urinals in the city treated in the same way, the yield would be about 79,000 lbs. of nitrogen, from which about 176 tons of sulphate of ammonia would be obtained, which, at £22 per ton, would represent a value of £3,872. The urine must be obtained pure, or nearly so, if the process is to pay; if there is a large addition of water the cost of carriage and manufacture will be so high that no profit will be realised. An attempt has been made on a large scale in Paris to extract ammonia from the sewerage, but the results have not been satisfactory.

W. H. T.

Cylinder-Condensation in Steam-Engines.

By C. L. GATELY and ALVIN P. KLETZSCH.

(Journal of the Franklin Institute, Oct. Nov. Dec. 1885, p. 275 *et seq.*)

This communication was presented to the American Association for Advancement of Science, with an introduction by Professor R. H. Thurston, who refers to a Paper of his published in 1882, in

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which he remarked that the function which determines the waste by cylinder-condensation "is evidently determined by the area of the cylinder-surface, time of exposure, variation of temperature of surfaces, and quantity and character of the working steam." He concluded that it was practically correct to take the amount of loss due to the cause above referred to, as varying as the square root of the ratio of expansion. An opportunity to investigate the subject more completely and more systematically than it had yet been possible finally offered in the spring of 1884, when Messrs. Gately and Kletzsch obtained a steam-engine of 250-HP. at Sandy Hook, Connecticut, for purposes of experiment.

The engine is of the Harris-Corliss type; the cylinder is 18 inches in diameter, with a stroke of 42 inches, fitted with a jet-condenser and a 9-inch air-pump. The exhaust is closely connected by a 7-inch pipe to the condenser. The steam-pipe is 5 inches in diameter, clothed in hair-felt and canvas. The cylinder is not steam-jacketed, but is covered with a non-conducting substance and laths. The work was taken up by a friction-brake. The engine was tested for leakage and proved to be absolutely tight. Steam was supplied from two or three boilers, cylindrical, 3½ feet in diameter and 17½ feet long, with a horizontal furnace-tube, and thirty-seven 3-inch flue-tubes. The heating surface of each boiler is 618·7 square feet. The methods and apparatus adopted for conducting the experiments are described in detail. The tests lasted for the most part two hours. The conditions for each trial were adhered to as closely as was practicable.

The conditions for the first case, or series of trials, were: the use of the condenser, with constant pressure, constant speed, and variable cut-off. The pressure in the boiler was maintained at about 60 lbs. per square inch above the atmosphere, and the speed at 68 revolutions per minute. The cut-off and the percentage of cylinder-condensation, or of steam not in evidence by the indicator at the point of cut-off, were as follows:—

Revolutions per Minute.	Cut-off.	Total pressure in Cylinder at Cut-off.	Cylinder-Condensation.
68·26	Per cent. 58·9	lbs. per sq. inch. 61·54	Per cent. 22·73
67·45	44·3	68·34	27·08
67·32	33·0	62·10	33·87
68·95	13·1	49·11	50·07

showing that the condensation increased rapidly with expansion of steam. It is shown, too, by the Authors that the curve derived by plotting these values is hyperbolic, indicating that, as Professor Thurston had before concluded, the condensation varied as the square root of the periods of admission.

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For the second case, the conditions aimed at were: the condenser in action, with variable pressure, constant speed, constant cut-off at one-fifth, or 20 per cent.:—

Revolutions per Minute.	Cut-off.	Effective pressure in Boilers.	Total pressure in Cylinder at Cut-off.	Cylinder-Condensation.
	Per cent.	lbs.	lbs.	Per cent.
69·02	20·8	80·0	78·80	35·24
70·72	20·6	66·85	66·89	47·83
71·73	24·4	52·33	53·21	36·84
68·91	21·02	37·0	39·83	41·43
67·92	24·2	22·3	26·74	41·19

showing that the condensation increased slightly as the pressure was reduced. In the last series of observations, an error appears to have been made.

For the third case, the experiments were made without the use of the condenser; but otherwise under the same conditions as for the second case, namely, variable pressure, constant speed, and constant cut-off—about 40 per cent.:—

Revolutions per Minute.	Cut-off.	Effective pressure in Boilers.	Total pressure in Cylinder at Cut-off.	Cylinder-Condensaton.
	Per cent.	lbs.	lbs.	Per cent.
67·98	41·2	60·15	65·36	10·85
68·57	42·0	44·09	50·42	23·51
67·51	40·1	33·50	(?)[1]	15·92
66·52	46·6	21·69	28·40	27·34

In the second of these trials, the percentage of condensation was probably too high. The three other results of trials show a decidedly increasing condensation, as was observed when the condenser was in action.

For the fourth case, the influence of speed, which was varied, was tested, in connection with constant pressure in the boiler, a constant cut-off, and the condenser. Steam was admitted for nearly the whole of the stroke:—

[1] Type missing in the original article.

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Revolutions per Minute.	Cut-off.	Effective pressure in Boilers.	Total pressure in Cylinder at Cut-off.	Cylinder-Condensation.
	Per cent.	lbs.	lbs.	Per cent.
62·98	93·8	19·05	27·38	24·37
50·3	96·1	19·04	28·35	28·75
33·74	98·1	19·67	28·53	33·51

Equations for the condensation in each case are deduced from the results of the several observations, which have been plotted for the purpose.

This investigation, Professor Thurston remarks, although most complete in plan and systematic in its prosecution, is but a single step in the work.

D. K. C.

Steam-Engine using Petroleum Fuel.

(Journal of the Franklin Institute, October 1885, Supplement, p. 10.)

A small steam-engine, using petroleum as fuel, suited for use in the lecture-room, is constructed by Queen and Co., Philadelphia. The boiler consists of a cast-iron water-tank, from which a group of fifty horizontal cast-iron water-tubes project, 1¼ inch in diameter and 9 inches long, surrounded by a double sheet-iron jacket open underneath, and connected at the upper side to a flue-pipe led to the chimney. Among these tubes, the flame from a petroleum atomiser is driven by steam direct from the boiler, controlled by a pressure diaphragm, by which the steam is shut off when the pressure rises above the normal force. The engine is inverted, having two single-acting cylinders end to end above the crank, with a steam-chest and a cock-valve between them. Neither the piston nor its rod is packed. The connecting-rod is pinned direct to the lower piston. The cut-off is automatic, by a single eccentric controlled by centrifugal force against a spring. The steam is exhausted through annular ports, which are uncovered by the pistons at the ends of the stroke.

When tested, the engine maintained a pressure of 75 lbs. per square inch, running at 500 turns per minute. Four-tenths of a horse-power was developed, with a consumption of 5 lbs. 14 oz. of oil, and 26 lbs. of water at 70° Fahrenheit per hour.

D. K. C.

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Feed-Water, Boiler-Scale, and Scale-Prevention.[1]

By W. GYSSLING, of Munich.

(Gesundheits-Ingenieur, 1885, p. 639.)

The feed-water may contain the following deleterious matters—scale-formers, fatty and corrosive substances, and air and other gases.

A. *Scale.*—Water is seldom entirely free from substances tending to form scale; these consist mainly of lime, gypsum, and magnesia. The only water really pure is that obtained by the condensation of steam. The above substances may be partly or wholly dissolved in the water. They may be deposited either as finely-divided solid matter, in the form of mud, or as a hard layer on the sides and bottom of the boiler. A special form of scale is liable to fly off in splinters or fragments when the boiler cools, and to form hard lumps or cakes at the bottom of the boiler.

Scale causes injury to the boiler in various ways, as for instance by abstracting heat from the contents, and causing time and trouble for its removal. It may also occasion explosions by permitting the overheating of the metal plates. This substance may further lead to the stopping up of the inlets and outlets to the boiler; it may gradually spoil the valves and taps, and may even pass over with the steam into the cylinder, and dangerously interfere with the action of the slides and piston.

Scale may be guarded against in various ways; among others, by a proper selection of the feed-water; by purification of the same, and by frequent cleaning out of the boiler. With regard to the choice of water, chemical analyses should be resorted to. If various kinds of water are available, it may sometimes be possible, by alternating the supply, to avoid much of the evil. Special care should be taken to condense and utilize all the waste steam. Should the only water procurable be rich in scale-formers, a boiler so constructed as to be capable of ready inspection should be employed. As many impurities can be removed by simply heating the water, it will be found advisable to introduce in all cases some common form of feed-water heater.

The best mode of effecting the purification of the feed-water is to treat it before it enters the boiler, and for this purpose either carbonate of soda, caustic soda, or quicklime may be employed. It will be necessary to determine which substance to use by chemical analysis, for which purpose about 3 litres of the water should be forwarded to the laboratory, in a carefully cleaned and sealed vessel. The boiler may be cleansed by hammering or chipping off the scale; by blowing out the mud and semi-fluid deposit, and by the addition of water containing substances capable of dissolving the scale. Directions follow as to how best to accomplish this, and

[1] From a broadsheet issued by the Bavarian Boiler-Protection Association.

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what substances to employ. The frequency with which to undertake boiler inspections is stated, and the character of the different kinds of scale is explained.

B. *Fatty Substances.*—The feed-water, as a rule, contains fatty matters when it comes into direct contact with waste-steam from an engine whose valves and cylinder are oiled with grease, tallow, or oils of animal or vegetable origin. Feed-water impregnated with such greasy substances should be used with caution, as the fat may combine with finely-divided scale-formers, such as lime or magnesia, to produce a spongy soap-like mass, which attaches itself to the metal, and may lead to superheating of the plates. Moreover, fatty acids are liable to be formed in the boiler, which may occasion destructive corrosion at or about the mean water-level. To avoid this, it is advisable to lubricate the machinery only with good mineral oil, or to use a filter to free the feed-water from grease. The same effect may be also obtained by the addition of soda, but this is apt to lead to violent frothing and priming of the boiler-contents.

C. *Corrosive Agents.*—Many descriptions of water, especially that from mines, peat-bogs, chemical works, &c., contain free acids likely to attack and corrode the boiler-plates. The same action may arise from salts sometimes found dissolved in water, as for example, chloride of magnesium, and by using the foul water from dung-hills, sewers, &c. To guard against such risks, the addition of soda is advisable.

D. *Air and other Gases.*—All water contains more or less atmospheric air, which is expelled by boiling, and passes off with the steam. If no exit for the air is provided, it may cause bubbles, air-spaces, or cavities to form below the water-level, and lead to the burning of the plates. Carbonic acid is also found in abundance in some kinds of water, and may be productive of similar evils. To avoid the damage likely to be caused by the presence of air, the following precautions may be mentioned. Proper construction of the boiler, so that all pockets, or places where the air-current cannot freely enter the steam-space, may be eliminated. Arrangements for active circulation of the boiler-contents, with the upper or lower heating-spaces as near as possible to the main boiler. Removal of the air from the water by preliminary heating, &c.

Scale-preventers.—All patent nostrums are condemned. (Here follows a list of twenty-seven specified, two of which are English.) Boiler-owners should not be misled and lulled into a false security by trusting to vaunted remedies, which rely for their action upon soda, &c., which it is better to employ directly, under proper advice.

G. R. R.

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Rösing's Steam Lead-Pump.

(Berg- und Hüttenmännische Zeitung, vol. xliv., p. 522.)

This apparatus has been in use for some months past at the Friedrichshütte, in Upper Silesia, for transferring molten lead from the final pots to the moulds both in the Pattinson process and that of desilverizing by zinc. It is continuous and automatic in action, and almost entirely dispenses with the troublesome, costly, and tedious operation of hand-ladling.

The apparatus consists of a pump-barrel, without plunger or piston, closed at the top, and having a ball suction-valve at the bottom. The top cover is perforated by two pipes, one serving for the introduction of steam, and the other, which reaches nearly down to the bottom of the cylinder, as a rising pipe. The latter is bent over into a goose-neck externally, so that the fluid lead may be delivered into a gutter communicating with the moulds, which are arranged in a semi-circle in the usual way, the gutter being pivoted, so that it may be brought opposite to each mould in succession. The steam-pipe is connected with a flexible hose to the feed-pipe which supplies the steam required in removing the last traces of zinc from the lead in the zincing process. The admission is regulated by a three-way cock, whereby the interior of the cylinder may be placed in communication with the boiler or the external air, or shut off from both, as required. The apparatus is portable. When in use it is suspended by a chain from a travelling crane, and is temporarily fixed in position by bars passing through lugs on the side, which are clamped to the edge of the pot.

The action of the pump is as follows: When it is lowered into the lead bath, which, following the usual practice in desilverizing, may be about 30 inches deep, and the interior of the cylinder is in communication with the air, the lead-pressure is sufficient to open the suction-valve, and the metal rises to the level of that in the pot. If now steam of one atmosphere excess pressure be admitted, the suction-valve closes, and the enclosed lead is forced up the rising-pipe until the lower end of the latter is exposed, when the steam-pressure suddenly falls, as the supply-pipe being small, it can escape freely through the much larger section offered by the rising-pipe. In consequence of this, the external lead-pressure opens the valve, and the fluid metal, in accordance with well-known physical principles, rises to a somewhat greater height than that corresponding to the difference of pressure covering the end of the rising pipe, and cutting off the freer exit for the steam, which consequently rises in pressure, and the action is repeated so long as a sufficient depth of lead remains in the bath. In practice it is found that the pots may be emptied to within about 5 cwts., which must be ladled out by hand. The pump may also be worked in another way by turning the three-way cock so as to give free communication with the atmosphere, and stop the steam-supply

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at the moment that the rising-pipe is emptied. This, of course, will save steam, but the action is no longer automatic. Under the latter condition the operation is repeated about thirty times in a minute, about 4 kilograms of lead being ejected each time. With steam of 1½ atmospheres excess pressure about 7½ tons of lead are lifted to a height of 4 feet per hour. The consumption of steam is said to be very small. The cost of the pump is about £17 10*s*. Before using the pump it is desirable to allow steam to pass freely through it for a short time to remove any condensed water, which might cause an explosion. The surface of the lead bath may be covered with an iron cover for the safety and convenience of the workmen. But if there is any chance of the metal not flowing freely, whether from deficient purity, or from being too cold, it is well to leave the neck of the delivery pipe exposed to the radiant heat of the fluid metal below to prevent the lead freezing. The dimensions of the various parts are determined by practical considerations, and may differ in each case, but must be so chosen as to ensure the proper and automatic action without special regulating valves, or gear, which are quite impracticable in the handling of such a fluid as very hot lead. A certain quantity of oxide of lead accumulates in the interior of the cylinder so that the pump requires cleaning about once a fortnight, but no other repairs have been found necessary after several months' working.

H. B.

Hydraulic Pumping-Machinery. By Eugéne Biver.

(Le Génie Civil, 1885, p. 81.)

This pumping plant, by Messrs. Hathorn, Davey and Co., of Leeds, erected at Lhuillier, Fuveau, by the Société de Charbonnages des Bouches-du-Rhône, was designed to complete the draining of the Castellane-Léonie section.

The question to be solved was :—Given a shaft already encumbered with mining-plant, with two forcing-pumps 20 inches and 16½ inches diameter, with wooden pump-spears, beams, guides, ladders, &c., to double the power by means of apparatus occupying as little space as possible, giving a higher dynamic efficiency, and in no way interfering with the existing machinery, which was to preserve the galleries from being inundated during the installation of the new engine. Other circumstances added to the difficulty of the problem. At the pit mouth, on the surface, was a Bull engine, with its cylinder placed vertically over the pit, also a platform and a winding engine. In short, the new installation was not to interfere in any way with the existing machinery, or with the future deepening of the pit.

The first scheme proposed was the installation at the bottom of the pit of a direct-acting rotative engine, taking steam from boilers on the surface, or from a battery of boilers near the engine in the mnie itself. In the first instance, the result must have been con-

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siderable condensation and heating of the pit; in the second, defective installation of the boilers. Each case required a room beyond the reach of the water, but this condition could not be fulfilled, and it was necessary that the pumps should be capable of working under water. Messrs. Hathorn, Davey and Co. being consulted, proposed the transmission of power to the pumps by water with pressure-engine on the surface, and hydraulic pumping-engines at the bottom of the pit.

Like installations had been adopted under similar conditions at the mines of Mansfeld and at Saarbrücken.[1]

The installation may be defined as follows: An economically-working compound steam-engine, forcing water at 42 atmospheres by means of two double-acting plunger pumps. At the bottom of the pit duplicate hydraulic pumps, both worked from the surface-engine pressure-water pipe, and a return-pipe, also a delivery-column from the hydraulic pumps to the draining gallery of the mine. The top of the pit is 292·805 metres (950 feet) above the sea-level. The draining-gallery is about 60 metres lower, or 232·820 metres above the sea-level; the situation of the hydraulic pumps 140 metres. The total elevation of the water is, therefore, 232·820 − 140 = 92·820 metres, or in round numbers 305 feet.

The work to be done was raising 7,700 litres (1,694 gallons) per minute

$$\frac{93 \times 7{,}700}{60 \times 75} = 159{\cdot}10 \text{ HP.}$$

The motive-power engine is of the compound type, having two horizontal cylinders with an intermediate receiver and cranks at 90°. The motor is calculated to supply 2,357·7 litres of water per minute to the hydraulic engines at the bottom, under an effective pressure of 42 atmospheres. The high-pressure cylinder has a diameter of 0·762 metre (30 inches), the low-pressure cylinder of 1·270 metre (50 inches), with a stroke of 0·914 metre (36 inches). The pistons are connected to a double rod, the front rod ending with the cross-head of the engine, the back rod connected to the plunger rod of one of the forcing-pumps. The front rods are 0·120 metre (4·72 inches) diameter, the back rods 0·080 metre (3·15 inches).

The forcing-pumps at the surface are two in number, and exactly alike. The plunger of each is attached to the piston of the motor.

The effective surface of the plunger is equal to the difference of the area of the plunger and the rod, that is, $S = 234{\cdot}75^2 c$. The capacity created by a full stroke is—

$$0{\cdot}023475 \times 2 \times 0{\cdot}914 = 42{\cdot}9\,l;$$
$$\text{for both plungers} \qquad 85{\cdot}8\,l.$$

The water necessary for the normal feed to the hydraulic engines must be 2,357·7 litres (519 gallons) per minute. The number of

[1] Minutes of Proceedings Inst. C.E., vol. xl., p. 294; vol. xliii., p. 405.

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complete strokes per minute with the steam-pistons will be $\frac{2,357 \cdot 7}{85 \cdot 8} = 27 \cdot 5$, corresponding to 27·5 revolutions of the fly-wheel.

Between the two engine-foundations, and in a line with the fly-wheel, is the steam-accumulator or regulator, in direct communication with the forcing main. The two steam-cylinders, though neither of them is steam-jacketed, are joined by a cast-iron pipe 10 inches in diameter, forming the intermediate reservoir, and connecting the valve-chest of the large cylinder with the exhaust of the small cylinder. The capacity of the reservoir thus formed is 640 litres. The relative volumes formed by the pistons in the two cylinders is 1 : 2·8. The cut-off in the large cylinder is fixed, and corresponds to an angle of 110°. The low-pressure diagram shows that the cut-off is equal to 1·09. It may thus be neglected. The small cylinder is provided with an expansion gear variable by hand. The speed of the engine on the surface must be regulated to suit the speed of the hydraulic underground. Underground the work is continuous; it must therefore be continuous on the surface. There must be no reservoir of work, such as is characteristic of the Armstrong system. An accumulator with such a reservoir of work would not allow the engine on the surface to determine the speed of the hydraulic pumps underground; that it should do so was a necessary condition, because the hydraulic pumps have sometimes to work under water. To render the action constant, the apparatus called a regulator is introduced. In similar installations in America, the Davey apparatus includes not a regulator but a simple reservoir of air. This gives excellent results in working, but has the following disadvantages:—

1st. Its size must be considerable.

2nd. It must have a large and considerable supply of air. The density increases with the pressure.

3rd. The bursting of this large reservoir, containing a pressure of 50 atmospheres, would cause a great explosion.

The steam-regulator is composed of two cylinders, and of two pistons mounted on one rod. In the first cylinder, the back surface only of the piston is subject to the action of steam. The motive-water in the pressure-pipe enters the second cylinder, and acts on the piston in an inverse direction to the steam. The areas Ω and ω of the pistons are calculated of course to satisfy the equation $f\Omega = F\omega$, f and F being the respective pressures of steam and of the water in the pipe. This apparatus is very sensitive. It serves as regulator for the compound motive-power engine. The piston-rod of the regulator is connected directly to the throttle-valve of the engine. In case of the breaking of the pressure-column of water, this safety apparatus prevents all further accident. The steam-regulator offers from a practical point of view other advantages. The effective pressure of the water in the supply-pipes may be varied by simply adjusting a reducing valve in the steam-pipe supplying the steam accumulator.

The engines at the bottom of the pit are twin engines, motors

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and pumps, working with a pressure of 42 × 15 atmospheres, and with the return water at a pressure of 15 atmospheres (corresponding to the difference of the level between the forcing pumps on the surface and the engines at the bottom, that is, 153 metres). The hydraulic pumps raise the water from 140 metres to a height of 232·82 metres to the draining-gallery. The delivery per minute is 7,700 litres; that is, 462 cubic metres (101,640 gallons) per hour. The stroke of the pumps is 1·829 metre (6 feet). The diameter of each pump-plunger is 0·463 metre (18¼ inches); the diameter of the rod being 0·102 metre (4 inches). To deliver 7,700 litres per minute, the two pumps must thus make 6·4 complete strokes. The corresponding speed of the piston, per second, is 0·39 metre (1½ inch). The difference of area is $0{\cdot}050364^2 - 0{\cdot}035200^2 = 0{\cdot}015164^2$ metre.

Trial of the Engines.—The diagrams taken at the time of the trial, August 29th, 1884, shows considerable variation in the pressure of water at different points of the circuit.

On the surface, the variations are more felt in the pump worked by the piston of the small cylinder, than in those which are worked by the large cylinder. According to the diagrams, the maximum oscillations correspond to variations of 35 to 54 kilograms per square centimetre (500 to 770 lbs. per square inch).

At the time of the trial, the water pumped from the mine was not considerable, the fly-wheel of the compound engine making only 22 revolutions per minute, corresponding to a delivery to the pressure-pumps of 1,887·6 litres per minute.

Each full stroke of the hydraulic engines at the bottom consumes 368·40 litres of motive-water, the number then of double strokes of the underground pumps must be—

$$\frac{1{,}887{\cdot}60}{368{\cdot}40} = 5{\cdot}12.$$

Five strokes only were recorded, the remainder was lost by the stuffing-boxes, distribution-valves, &c.

The corresponding delivery of the pump was—

$$1{,}201{\cdot}80\,l \times 5 = 6{,}009 \text{ litres per minute.}$$

The effective work in water raised was then—

$$100{\cdot}15 \text{ kilograms} \times 93 \text{ metres} = 9{,}313{\cdot}95 \text{ kilogrammetres} = 124{\cdot}20 \text{ HP.}$$

According to the diagrams, the total work done by the steam-pistons was—

$$T_m = 229{\cdot}20 \text{ HP.}$$

Since the useful work done was $T_u = 124{\cdot}20$ HP.

$$\frac{T_u}{T_m} = 0{\cdot}542 = 54{\cdot}2 \text{ per cent.}$$

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The work of the air-pump in raising the condensing water to the cooling pond (an amount of work which slightly increases the efficiency) is not taken into consideration.

The Directors of the Société de Charbonnages des Bouches-du-Rhône intend to make use of the installation for working, independently of the pumps described, some underground hydraulic motors.

It will be only necessary to increase the number of strokes of the compound engine on the surface to provide a larger volume of water under pressure, from which it will be possible to work additional pumps in the "dip," and also hauling engines. For hauling, rotative hydraulic motors, with two cylinders, having cranks coupled at right angles, and valves worked by means of eccentrics, will be employed.

The cost of the installation was as follows:—

	Francs.	
Surface engines. Compound-engine and forcing-pumps free on board in London	37,036	70
Hydraulic engines at the bottom. Transport, marine insurance	37,415	95
Loading and unloading	13,313	26
Cast-iron pipes (61,997 kilograms) and transport . .	35,782	12
	123,548	03
Total cost of the whole installation	215,771	20

Or say £8,628.

H. D.

Theory of the Sliding-Friction of Rotation.

By Robert H. Thurston.

(Transactions of American Society of Mechanical Engineers, vol. vi., 1885, p. 122.)

The Author in this Paper investigates journal-friction under three typical cases: 1st, when the journal is initially fitted to its bearing, and so perfectly adjusted that no sensible wear takes place; 2nd, when wear occurs, and until this ceases to be observable by reason of a "fit" being attained; 3rd, when the journal is loose and the bearing-surface is assumed to be a straight band of inconsiderable width. Recent investigations concerning the action of lubricants prove that the amount of friction is not necessarily measured by the product of the load into the coefficient of friction. Thus it is necessary to examine the way in which intensity of pressure varies with different modes of fitting journals, and the manner in which that variation affects the total resistance due to friction.

After considering in general terms the work done by friction, and consequent heat developed and temperature attained, Case I. is examined. This is the most interesting, especially as the custom of previous grinding and scraping of journals is becoming

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more and more common. The result of the investigation shows that the work of friction is

$$U = 2{\cdot}5\, f n r t \pi W = 7{\cdot}86\, f n r t W.$$

Where W is the load, f the coefficient of friction, r the radius of journal, n the revolution per unit of time, t the time taken—

Case II. gives

$$U = 10\, f n t r W \text{ nearly.}$$

Case III.

$$U = 2\pi \sin \phi\, n t r W$$
$$= \frac{2 n t r^2 W}{\sqrt{1 + f^2}}$$

This may be taken as nearly equal to

$$U = 6{\cdot}3\, f n r t W.$$

So that for the three cases the results of the Author appear to show the ratio of work lost in each case respectively is as

$$7{\cdot}86 : 10 : 6{\cdot}3.$$

Thus the last case has minimum friction, but if approached too closely and the surface of contact be too narrow (*i.e.*, approach the ideal case of a line of contact), rapid wear and heating result, and consequent cost and damages are increased.

H. S. H. S.

The Bursting of Fly-wheels. By — Köchy.

(Verhandlungen des Vereins zur Beförderung des Gewerbfleisses, 1886, p. 25.)

The bursting of fly-wheels of rolling-mill engines had become so frequent that in 1883 the Prussian Government authorities directed a searching inquiry to ascertain the cause in detail. The official returns, entrusted to the Author for examination, are the basis of this essay; the data and probabilities point to the conclusion that only a small proportion of the actual casualties have been scheduled. Grouping forty-one accidents, he finds the circumferential velocity of one wheel was 42 metres (137·8 feet), and of the others none exceeded 33 metres (108·27 feet). In only a few instances were the conditions abnormal. He believes, therefore, that a special danger exists in the ordinary employment of rolling-mill engine fly-wheels. No accident was attributed to shocks, to which they are necessarily subject when at work, and the significance of fracture in the arms and in the hub does not appear to have attracted sufficient attention. Accidents fatal to life were few, but buildings and machinery had been seriously damaged or destroyed. He recommends proprietors of works to pay special attention to every detail of these fly-wheels. The disruptive force active in a revolving fly-wheel results from: (1) The static weight;

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(2) the centrifugal force; (3) the torsion moments due to acceleration or retardation at the axis of ordinary fly-wheels (which are neither pulleys for belts or ropes, and not fitted with toothed gear); (4) the tangential forces acting in a plane perpendicular to a section of the rim in the case of belt, rope, or toothed-gear transmission.

Consideration of the static weight at once leads to that of the preponderating centrifugal force developed in the rim and in the arms. Diagrams illustrate the composition of the elementary forces, and a series of equations results in fundamental formulas, which the Author applies to a numerical estimate of the stresses which would fracture fly-wheels of the dimensions and in the circumstances reported, and which were actually destroyed; taking three examples, a wheel at the Bürbacher forge,[1] a wheel at Königshütte, a wheel at Witten, and, as a fourth example, the pattern of a wheel frequently adopted in works where no similar disaster has occurred. To these he recurs throughout the essay. All four wheels, although in different localities, were subject to the same stress on the material at about equal circumferential velocities, from which it appears that experience has determined a certain uniformity of practice in proportioning such wheels; moreover, a stress equal to the breaking load, or even to the elastic limit of the material, would only accrue, at much higher velocities than usual, if there was carelessness on the part of the engineer in charge, and it is hardly possible that the three wheels were destroyed by centrifugal force, if the joints had the strength due to the dimensions of the arms and the rim. The effect of the position of the rim-joint, the mode of construction, and shape of the joint of the arms with the rims, is particularised in this connection.

Two diagrams of the stress developed in a particular wheel by the centrifugal and by the retarding force are appended.

Fly-wheels (except those which transmit force from their circumference to other pulleys or wheels) are subject to alternating forces at their axis, due to variations in the motive force in the engine and to resistance, the latter in the nature of shocks, when iron enters the rolls it is difficult to measure, but it may be sometimes very dangerous. Deformation of the rim occurs when the angular motion is retarded; the greatest centrifugal load is then at the junction of the arms with the rim. The greatest stress on the arms is at the hub, but they are usually tapered, preserving at least uniformity of strength. A dovetailed wedge-shaped junction of the arm and rim tends to turn about one of its angles, and in course of time to work loose and strain the cheeks. Any form of attachment is more or less open to the same objection, but the strength of the rim is not much reduced in consequence. The Author advocates the use of wrought iron for the material of the arms.

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[1] "Zeitschrift deutscher Ingenieure," 1872, Heft ii., s. 97, Krüger.

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No particulars are to hand which would enable him to calculate the effect of sudden retardation at the rim of belt and toothed-geared fly-wheels; he proposes to deal with the subject in a separate essay. Premising that such wheels are usually made considerably heavier than ordinary fly-wheels, that the conditions in which they work are much more favourable to resist shocks, and that they should be preferred, provided the construction and fitting, which necessitates greater precision, is altogether satisfactory.

Ordinary fly-wheels are more numerous than wheels in the particular service he has considered, but they are equally liable to accident if the proportions and details of construction to which he has animadverted are improper, or they are allowed to acquire an inordinate velocity. He points out that his calculations do not, on the whole, conflict with the rules of practice, and this he takes as corroboration of accuracy—that fly-wheels of good construction, properly looked after, would not fracture; but it is nevertheless true that wheels have broken up, and there are many now at work in hazardous condition.

He concludes with a series of recommendations. The following are the most noteworthy:—

The arms should be of wrought iron, each, and its attachment of equal strength with the iron rim.

The engine should be fitted with a quick-acting governor, driven direct from the shaft; the intervention of a friction-pulley, or belt would be objectionable.

The work at the rolls must be conducted with circumspection. If a wheel is suddenly brought up, or its velocity greatly checked, fracture may be expected. It must in this event, before it is set in motion, be minutely examined, to ascertain whether it is intact.

A competent engineer, or the proprietor himself, should examine the fly-wheels periodically.

Avoidance of all forms and modes of construction which might create strains in those parts which are loaded when at work, whether the wheels are built up or cast in a single piece. Many fractures may be attributed to the employment of solid hubs, which are subject to initial strain from the casting.

Official and managing engineers are expected to avail themselves of the calculations and information submitted for the critical examination of wheels at work, not only as a matter of interest, but as tending to avoid accidents which otherwise, he anticipates, might sooner or later occur.

B. A.

Improvements in Teledynamic Transmission. By Prof. REULEAUX.

(Verein für Eisenbahnkunde. Protokoll, 10 Nov., 1886.)

The problem how to transmit power to a distance has found many practical solutions. The writer would not employ an electric current, remarking that power is converted into current, and

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current into power (small into great velocity and great to small) with notable loss of efficiency, and that costly machinery is required. He criticises hemp-rope transmission as the fashion of the day, and proceeds to discuss, analytically, the conditions which obtain in belt, wire-rope, rotating-shaft, and high-pressure hydraulic transmission of power. He mentions works in which wire-rope transmission has been adopted for large power (particularly by Ziegler at Schaffhausen, &c.); then, assisted by diagrams, explains a novel system of his own design, with many modifications, which would, he says, be cheaper to construct, easier to maintain, and more efficient, obviating defects which he notices in other rope-transmissions. He proposes to employ steel wire, of light section, travelling at a high velocity, and as far as possible in one length, continuous throughout a circuit system. The intermediate pulleys are bearers only, consequently light, of a particular dished pattern, which he figures. Deflection of the wire in any direction is effected by pulleys in proper angular adjustment without the intervention of gearing. Stations outside the main circuit are served by external loops of the main wire; the height at which the wire is carried is modified by local circumstances, and compensating pulleys are now provided maintaining their position by gravity and a constant pre-arranged tension in the wire. In a line system the driving-wire runs over bearing-pulleys on alternate standards, and the catenary depression of the driving and driven length of wire is thus equalized. He would provide motive power to all machines within a mill by a circuit of continuous wire rope, driven by a main pulley in the primary circuit. The Author states that hydraulic high-pressure (50 to 150 atmospheres, $\frac{1}{2}$ to 1 ton per square inch) transmission from a central station may be used in "circuit" to give out power at any point, beyond which the water may be at the same or at diminished pressure, but still the full quantity, returns to the starting-point. Where a machine is stationed he has a by-pass with special arrangements for admission of water to the machine, and adapts the machine itself to work, subject to the static pressure on the pipe. Used in line, the hydraulic pressure is utilized, and the water then runs to waste. To meet the requirements of practice he recommends a combination of the two systems, and notices that hydraulic transmission is convenient on railway premises, &c., for cranes, hoists, &c.

The writer elaborates formulas for computing the dimensions of a transmitter, and shows the convenience of two critical qualities, the specific load (N_s) and the specific value (N_s) of the transmission :—

N = the number of P.S. (HP.) transmitted;

S = permissible stress per square millimetre (0·00155 square inch) unit section of the material;

τ = modulus of tension { P = circumferential force acting on driven pulley; t = tension of transmitter running on " ; T = " " " off " ; then $T > P$; by a quantity > 1, that is, by τ

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q = sectional area in square centimetres (0·155 square inch);
v = metre (3·28 feet) unit velocity of the transmitter;
A_o = 10 metres (32·8 feet) a unit-length;
G = mass of the material in the unit-length A_o;
σ = the specific weight of the material;
N_o = the specific load = the number of P.S. (HP.) transmitted per centimetre (0·155 square inch) per metre unit velocity (196·8 feet per minute);
N_s = the specific value = the number of P.S. (HP.) which a unit weight 1 kilogram (2·2 lbs.) of the material renders in the unit distance A_o 10 metres (32·8 feet).

(1) $N = \frac{4}{3}\frac{S}{\tau} q v.$ (3) $G = 10\, A_o \frac{q}{100} \sigma.$

(2) $N_o = \frac{N}{q v} = \frac{4}{3}\frac{S}{\tau}.$ (4) $N_s = N_o \frac{v}{\sigma}$; also $\frac{2}{3}\frac{S}{\sigma} v.$

(5) $N_s = \frac{N_o}{2}\frac{v}{\sigma}.$

(4) is applicable in all cases; (5) applicable to "line" belt- and rope-transmission where the draught is on both lengths.

The specific load N_o of a belt or other transmitter should be known, and might with advantage be stated in trade lists; the better the material the higher its specific load. The average values are—

Cotton belt	$N_o = \frac{1}{7}$	Steel pipes	$N_o = 2\frac{3}{4}$ to 4
Rubber	$= \frac{1}{6}$	Steel shafting	$= 1$ „ 2
Leather	$= \frac{1}{5}$	Iron wire	$= 4$ „ 5
Wrought-iron pipes	$= \frac{1}{3}$ to 2	Steel wire	$= 8$ „ 10

The specific value is high when high velocity and great tension are combined; if the product of these factors is small the value is low.

Resistance and loss of power in transmission must be estimated for each case; we know that the larger the specific value of the system the smaller the loss. With shafting the loss may be 50 per cent.; in wire-rope transmission it has proved to be 14 per cent., but the writer believes that in his system it might be reduced to 4 per cent.

In hydraulic transmission under high pressures the cost of the pipes is considerable, and that of the water may be disregarded; it is the pipes that have to be dealt with in the calculation. It appears that the specific load does not depend on the hydraulic pressure; if the velocity of flow is constant the HP. transmitted will be the same per unit of pipe section, assuming that its thickness is proportioned to a predetermined stress for the material employed.

A Table gives the ratio of efficiency in a few systems. The tension and velocity are less than the maximum which practice would justify. Steel wire rope in circuit-transmission shows to great advantage. If the power was large and a counter-pulley employed, it would contrast still more favourably.

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Transmitter.	v. Velocity in metre-seconds.	S. Kilograms per millimetre section.	N_a. P.S. (HP.) per centimetre, met. second velocity.	N_s. P.S. (HP.) per kilogram, per 10 metres length.[1]	Ratio. Efficiency.
					Per cent.
Steel-wire rope—					
Circuit-transmission .	30	15·0	10	38·46	100·0
Line- ,, .	30	15·0	10	19·23	50·0
Wrought-iron wire rope—					
Circuit-transmission .	30	7·5	5	19·23	50·0
Line- ,, .	30	7·5	5	9·61	25·0
Belts—					
Line- ,, .	30	0·38	$\frac{1}{3}$	6·00	15·6
Hemp rope—					
Line- ,, .	30	0·17	$\frac{1}{12}$	2·50	6·5
Hydraulic high-pressure—					
Steel tube	4	6·0	4	2·05	5·3
Iron pipe	4	3·0	2	1·03	2·7
Steam—					
Steel tube	4	3·0	2	1·03	2·7
Cast-iron pipe . . .	4	1·0	$\frac{2}{3}$	0·34	0·9
Shafting—					
Wrought-iron or steel .	1	3·0	2	0·26	0·7

[1] Measured between the source of power and the point of delivery.

Transmitter.	v. Velocity per minute.	S. Lbs. per square inch sectional area.	N_a. HP. per square inch per 196·8 feet velocity per minute.	N_s. HP. per lb. per length of 66 feet.	Ratio. Efficiency.
	Yards.				Per cent.
Steel-wire rope—					
Circuit-transmission .	1,968	21,334·2	63·7	35·81	100·0
Line- ,, .	,,	21,334·2	63·7	17·66	50·0
Wrought-iron wire rope—					
Circuit-transmission .	,,	10,667·1	31·85	17·66	50·0
Line- ,, .	,,	10,667·1	31·85	8·56	25·0
Belts—					
Line- ,, .	,,	540·47	1·27	5·51	15·6
Hemp rope—					
Line- ,, .	,,	241·79	0·53	2·29	6·5
Hydraulic high-pressure—	Feet.				
Steel tube	787	8,533·7	25·48	1·87	5·3
Iron pipe	,,	4,266·8	12·74	0·94	2·7
Steam—					
Steel tube	,,	4,266·8	12·74	0·94	2·7
Cast-iron pipe . . .	,,	1,422·28	4·24	0·313	0·9
Shafting—					
Wrought-iron or steel .	197	4,266·8	12·74	0·236	0·7

B. A.

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Application of Electricity to the Transmission of Power.

By — FAVÉ.

(Comptes rendus de l'Académie des Sciences, vol. cii., 1886, p. 97.)

The cannon foundry at Bourges has employed since 1879 two Gramme dynamos, one as generator, the other as receiver, for the testing of metals by means of a traction machine. The same establishment makes use, for the handling of very large ordnance, of two 20-ton cranes, each worked by a special motor, with current furnished from a single generator. The power is transmitted a distance of 120 metres, and is about 13 HP. out of 25. The working has been always effected without accident.

P. H.

Belgian Mine-Regulations.[1]

Until the early part of 1884, Belgian mines were subject, not only to general regulations, but also to others, varying in each province, which were added to from time to time as occasion required, but many of which had become obsolete and contradictory. In 1879, however, a Royal Commission was appointed to revise and codify the regulations; and the new rules, of which the following is a short summary, received the king's assent on 2nd May, 1884:—

1. *Rules for ensuring safety in the ordinary working of mines.*— Plans of each seam or deposit, showing the advance of the workings, are to be kept up month by month; and copies, exchanged every six months made up to date, are to be sent to the government inspector. Every seat of working must have at least two distinct issues always accessible to the men; and the mouth of the ladder must be outside the surface buildings, while notice must be given to the inspector of any intention to abandon the working before it becomes inaccessible.

The third chapter regulates the ascent and descent of the men by ladders, buckets or cages, and provides for the signals for ensuring safety, as well as means for the prevention of overwinding.

Chapter 4, divided into four sections, deals with ventilation, lighting and the use of explosives, in connection with which, mines are divided into two classes, viz., those with and without firedamp, while the former are subdivided into those but slightly affected by firedamp, fiery mines, and those subject to sudden outburst of gas. All mines with firedamp must be worked, as far as possible, by successive downward stopes, ventilation, by a current passing downwards along the faces being expressly forbidden. The air-

[1] The original is in the library of the Inst. C.E.

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ways and return air-ways must be separated by rock of sufficient thickness to resist an explosion; while air tubes and partitions are only to be used in temporary works. In mines subject to sudden outbursts of gas, every driving must be ventilated by a current of fresh air, that has not passed through any working place. Boring in advance of driving is prescribed when there is reason to fear the tapping of a hollow containing firedamp. In mines subject to sudden outbursts, naked lights are forbidden on the surface near the mouths of the shafts. The framework of winding pit-heads must be made of incombustible materials, and not be roofed over. In all mines with gas, safety lamps of the Mueseler type must be employed, subject only to certain modifications which are enumerated. Lamps extinguished may not be re-lighted underground; and the possession of a pipe or any means of obtaining fire are strictly prohibited. The 4th section of this chapter regulates the use of explosives, and prohibits their use for getting coal in all mines with gas, as well as their introduction even into mines of the second and third categories, without previous authorization by the inspector.

Chapter 5 enjoins boring in advance when there is reason to apprehend an incursion of water, as well as the keeping of a register showing the position of all bore-holes. Chapter 6 provides for a register being kept of the hands engaged underground, their proper supervision and the responsibility of the supervisors; while Chapter 7 empowers the authorities of each province to grant temporary and conditional dispensation from the strict carrying out of the regulations.

2. *Special provisions for preventing accidents.*—In case of threatened danger, notice must be given to the government inspector, who shall concert measures for averting it with the manager. If the latter refuse to carry out the inspector's recommendations, he shall report to the provincial authority, which, after hearing the manager, shall decide upon the measures to be taken, subject to the approval of the Minister of the Interior, acting on the advice of the Council of Mines. In cases of urgency, the provincial authority, and even the inspector, may act on their own responsibility.

3. *Measures to be taken in the event of accident.*—Every fatal, or even serious, accident shall be reported to the inspector, who may also, if circumstances come to his knowledge by which life or the safety of the mine is endangered, inquire into the causes and report, and may also make requisition for tools, horses and men. Works for rescuing men or averting danger shall be conducted by the manager under the inspector's direction; but in the event of differing opinions, that of the inspector shall prevail. One or more surgeon-physicians shall be attached to every mine, which must be provided with medicaments and appliances for the first relief of the injured; and the owners of mines adjoining those in which an accident may have happened are required to furnish every means of assistance at their command.

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4. *General arrangements.*—Mine-owners are to provide the inspectors with everything necessary for their inspection, must produce all plans and registers, and afford every information required, while a book is to be kept for receiving the inspector's remarks and recommendations.

To these regulations are appended ministerial instructions as to the keeping up of plans, the dimensions of the standard Mueseler safety lamp, and the limits of departure permitted therefrom in manufacture.

J. W. P.

Shaft-Lining with Wrought-iron Rings and Oak Planking.

By — MALE, Chief Engineer of the Montieux Mines, Saint-Étienne, Loire

(Bulletin de la Société de l'Industrie Minérale, vol. xiv., 1885, p. 555.)

The north-west pit of the Montieux mines is sunk in very wet ground, only 60 yards from the Isérable brook, necessitating a very careful masonry lining for the first 24 yards. This lining, with a clear diameter of 3 metres (9 feet 10 inches), was carried on a stout oak curb 14 inches deep. Below this point the lining was constructed of oak planking 2 inches thick, supported by rings of wrought iron of [section, $7\frac{7}{8}$ inches deep × $3\frac{1}{8}$ inches wide × $\frac{3}{8}$ inch thick, weighing $17\frac{3}{4}$ lbs. per foot run. Each ring was in three segments butted together, fished at each joint by a cast-iron block exactly filling the groove, and secured by four dowel-pins passing vertically through both flanges, and through the fish-block. The first ring from the top was carefully levelled, and suspended from the oak curb by six distance pieces also of ⌴ iron, 4 inches × 2 inches × $\frac{3}{8}$ inch thick, weighing $9\frac{1}{2}$ lbs. per foot run, with a foot turned up at each end, by which they were bolted to the flanges of the rings. As the sinking progressed, each succeeding ring was similarly suspended from that immediately above it, and, as the distance pieces or bearers in each section were all exactly of the same length, no trouble in levelling the rings was experienced.

In ordinary ground the bearers were 2 feet $7\frac{1}{2}$ inches long, so that the rings were spaced 3 feet $3\frac{3}{8}$ inches (1 metre) centre to centre; but in passing through faults, or where there was much "thrust," they were spaced, say 2 feet 6 inches, or even 2 feet centre to centre, while their thickness was increased to $\frac{5}{8}$ inch. In solid ground they were spaced as much as 4 feet centre to centre, but the average distance, for the whole depth of 360 yards, was 1 metre (3 feet $3\frac{3}{8}$ inches). Even where the shaft passes through the Soleil fault, where there is great thrust, and the strata are sharply inclined, the rings are still as true as when they were put in.

The weight and cost of materials are as follows :—

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	Lbs.
Three arcs of ring ⅜ inch thick	569
Six distance pieces 2 feet 7½ inches long	190
Three fish-blocks	203
Twelve dowel-pins	26
Twelve bolts	15
	1,003

or, say 9 cwt. for one ring 9 feet 10 inches inside diameter.

	£.	s.	d.
Cost of one ring at £11 15s. per ton (29 francs per 100 kilograms)	5	5	9
Oak planking 2 inches thick, 10 square metres for each section, at 4 francs per metre	1	12	0
Wedges and packings	0	8	0
Cost per metre run	7	5	9

or, say £6 13s. per yard run.

The average rate of sinking, deducting the time occupied in fixing air-tubes, but including fixing the lining, was 22 yards (20 metres) per month.

As the sinking was followed up step by step by the lining, no temporary timbering was required, as would have been the case with a brick-lined shaft, while accidents from falls of rubbish from the shaft-sides were entirely obviated. Fixing the lining was executed as quickly as setting temporary timbering, so that the time usually occupied in bricking, one-third of the whole, was saved, effecting a corresponding economy in labour and establishment charges.

Another considerable saving results from the reduced thickness of this lining, as compared with brickwork. For a 9 feet 10 inches shaft lined with brick, the excavation must be 12 feet 2 inches diameter, corresponding to an area of 116 square feet; whereas the excavation for a shaft with the lining described above need only be 10 feet 10 inches diameter, with an area of 92 square feet, showing a clear saving of one-fifth in the excavation and in the spoil to be disposed of. The consecutive rings form very convenient stages for fixing scaffolding for any repairs that may be required, while a broken or defective ring can be speedily and easily replaced.

This system is specially adapted for trial-shafts, owing to the rapidity with which the work can be carried down, and the facilities which it affords for examining the successive strata; while, if unsuccessful, the whole of the materials can be safely and easily removed, beginning from the bottom, and are available for future use. In this way some 30 per cent. of the cost of the sinking may be recovered.

The work of sinking the north-west pit was let to three working contractors, each taking his turn with his own gang, for 135 francs per metre sunk (£4 18s. per yard). They found their own powder and fuzes, but all other materials and tools, and labour on the

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surface, was paid for by the company. The total cost of one month's work was as follows:—

	Francs.	
Twenty rings with planks, wedges, &c., at 183 francs each	3,660	
Twenty metres excavation, at 135 francs per metre	2,700	
Two engine men	270	
Four surface men	420	
One smith	100	
Forty tons coal at 10 francs per ton	400	
Wear and tear of wagons, tools, and general iron-work	100	
Hemp, oil, tallow, petroleum, &c.	90	
Town water for boilers	100	
Depreciation of two winding ropes	190	
Total for one month, 20 metres depth	8,030	(£318 13s. 6d.)

or £14 11s. 6d. per yard.

A brick-lined pit of the same diameter would cost at least 505 francs per metre (£18 6s. 8d. per yard), and would take one-third as long again to reach the same depth.

At Seraing, where this method was first introduced, it has been successfully employed in a pit of 14 feet 9 inches clear diameter, with rings $\frac{5}{8}$ inch thick. In downcast pits, even when wet, the oak planking is found to stand very well, but it is doubtful if it would be equally successful in resisting the combination of moisture and furnace gases often found in upcast pits.

Iron frames are also used at Seraing for supporting the roof of the main roads and chief galleries underground, and for that purpose are formed of old or rejected flat-footed rails. Each frame forms a horse-shoe shaped arch, with flat base, 6 feet 1 inch high × 4 feet 11 inches at widest part, and is in two parts, jointed about mid-height on each side by fish-plates and bolts. The frames are usually spaced about 1 metre (3 feet $3\frac{3}{8}$ inches) apart, their correct distance being maintained by tie-bolts and wooden struts. The roof and sides of the road are packed in the usual manner with wooden bars and slabs.

In 1870 a comparative trial, extending over seven years, was commenced in a long road, the maintenance of which had proved difficult. Twenty-one iron, and fifty wooden frames, the latter formed of oak posts 8 inches diameter, were employed with the following results, the bars and packing in each case being renewed once a year:—

Iron Frames.—First Cost.

	Francs.
Twenty-one frames (including tie-bolts) at 50 francs	1,050·00
Bars and packings	40·80
Oak struts between frames	24·00
Labour in setting	147·00
Total first cost	1,261·80

or 60 francs per lineal metre.

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MAINTENANCE FOR SEVEN YEARS.

	Francs.
Repairs to three broken frames	15
Renewal of bars and packings	980
Renewal of eight broken tie-bolts	40
Rectification of tram-road	20
Total maintenance for seven years	1,055

or 53 francs per metre for seven years, or 8 francs per metre per annum.

TIMBER FRAMES.—FIRST COST.

	Francs.
Fifty oak frames (8 inches diameter)	805
Bars and packing	68
Labour in setting	350
Total first cost	1,223

or 24 francs per lineal metre.

MAINTENANCE FOR SEVEN YEARS.

	Francs.
Total renewal of all woodwork once a year	8,561
Retification of tram-road twice a year	1,051
Total maintenance for seven years	9,611

or 192 francs per metre for seven years, or 28 francs per metre per annum.

As a main road must usually be maintained for twenty years before the coal is exhausted, the comparative cost per metre would be as follows:—

	Iron Frames.	Timber Frames.
First cost	Francs. 60	Francs. 24
Maintenance for twenty years	160	560
Total	220 (£8 14s. 7d.)	584 (£22 15s. 7d.)

The present low price of iron and steel rails would give a still further economy in the first cost, while the additional cross-section to which the timber-framed gallery must be excavated, and the fact that the wood frames are worthless at the end of the time, while the iron frames are little the worse, must also be taken into consideration.

The Paper is illustrated with Plates, showing different forms and arrangements of iron frames for shafts and galleries.

W. S. H.

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On Smelting Antimony in the Blast-Furnace.

By C. M. A. BALLING.

[Stahl und Eisen, vi., 1886, p. 62 (Chemiker Zeitung, 101).]

At Pribram antimony ores, both sulphides and oxides, with a considerable quantity of siliceous waste, are smelted in a blast-furnace for crude metal (star regulus), which is subsequently refined in a reverberatory furnace.

The composition of the materials forming the blast-furnace charge is as follows:—

	Dressed Ores.		Oxidized Ores.	Liquation Residue.		Flue Stuff.	Refining Slag.
	Raw.	Calcined.		Raw.	Calcined.		
Antimony	43·3	48·9	46·4	21·4	23·1	56·1	25·7
Iron	14·4	..	..	12·9	..	..	..
Sulphur	25·3	0·7	3·6	15·1	2·4	..	..
Ferric oxide	..	23·4	2·4	..	17·3	6·9	..
Silica	11·7	23·3	26·6	41·6	49·2	10·8	5·9
Alumina	0·3	4·0	..	0·5	2·4	..	..
Carbonate of lime	3·3	..	7·0	4·0	..	..	..
Lime	..	1·0	..	4·0	0·7	..	..
Sulphide of antimony	..	..	..	..	..	..	2·4
„ iron	..	..	..	..	..	..	53·4
„ sodium	..	..	..	..	..	..	9·0
Carbon	..	..	..	..	..	5·1	..

The blast-furnace has a round stack 6 metres high, 1·4 metre in diameter at the throat, and 1 metre in the hearth, which is closed, and has five water twyers and three outlets for the molten products—an upper one for the slag, and two at the hearth bottom. Of these latter, one communicates with the exterior by a passage having a slight rise and forms the ordinary tap-hole for the metal, while the other has a horizontal outlet, and is only used in blowing out the furnace. The blast, supplied by a Krigar screw-blower, amounts to 15 cubic metres per minute at 30 cubic metres water-pressure. The gases are collected by a tube leading from the side of the furnace a short distance below the throat. The furnace is kept in blast for three weeks continuously, and smelts 19·2 tons of materials daily.

The smelting mixtures are of two kinds, whose composition is as follows:—

	A.	B.
	Kilograms.	Kilograms.
Calcined dressed ores	550	600
„ liquation-residues	750	600
Ores balled with lime	200	..
Flue stuff „ „	100	..
Raw ore	..	100
Oxidized ore	..	100
Raw liquation-residues	..	100
Limestone	600	800
Foul furnace-slags	400	400
Refining slags	..	200
Crude antimony metal No. III	..	100

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The most favourable result is obtained when these mixtures are used alternately, two charges of A being followed by one of B.

Lime is used to the extent of 10 per cent. to ball up part of the washed ore to a compact mass, and some of the flue stuff is similarly treated, but with only 7 per cent. This is however only necessary at starting, as it is found that when the furnace has been blowing for eight or ten days the materials may be charged in powder without in any way injuring the working of the furnace.

The products of the blast-furnace are crude antimony metal, slag, and flue stuff. The first is divided into three classes, of the following average composition :—

	No. I.	No. II.	No. III.
Antimony	90·02	73·80	65·04
Iron	6·23	16·66	23·80
Sulphur	2·85	8·42	10·46

The first two qualities are sent to the refinery-furnace, while No. III. goes back to the blast-furnace. The relative proportions are: No. I., 82·5; No. II., 9·0; and No. III., 8·5 per cent. of the total output of the furnace.

The slags produced in the blast-furnace vary slightly in comparison with the smelting mixtures. Thus—

Silica	46·9	45·9
Lime	34·6	31·4
Ferrous oxide	15·1	19·9
Antimony	0·5	0·9

The first is produced from the mixture A, and the second from B.

There is very little flue dust produced in the blast-furnace, the bulk of the product being derived from the preliminary operation of calcining the dressed ores. This is done in a single-bedded reverberatory furnace 8 metres long and 2 metres broad, with five working doors on one side, heated with brown coal, which is burned upon a step grate. The produce is about 24 cwt. per day. The ore, which is charged in quantities of 4 cwt. at a time, clots and softens, giving off a large quantity of sulphurous acid as soon as it attains a low red heat, but subsequently becomes dry again, and when finished is in the state of powder. The average time that the charge remains in the furnace is twenty hours. The liquated residues are roasted in stalls upon a bed of old mine timber, the heap being allowed to burn for five or six weeks.

The refining of the crude blast-furnace metal is conducted in a reverberatory furnace with an iron bed 4 metres long by 2·5 metres broad, covered with a layer of fire-clay 28 inches thick. Brown coal is used upon an ordinary grate in the fireplace. The working door is one side, and the tapping-hole at the end of the bed. The gases on their way to the chimney pass under an iron plate below the furnace, upon which the moulds for the refined metal are placed in order to warm them. The charge consists of 9 cwt. of No. I. metal, 1 cwt. of No. II., the first being coarsely broken; while the

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second is in powder, to which are added 42 kilograms of sulphate of soda, 5 kilograms of charcoal dust, and 150 kilograms of unroasted ore. The operation lasts ten hours, and a final slag is made by the addition of 3·39 kilograms carbonate of potash, 2·61 kilograms carbonate of soda, 1 kilogram of raw and 6 kilograms of calcined crude antimony regulus (liquated sulphide of antimony), and 12 kilograms of the same slag from previous operations. The refined metal (star regulus) contains as impurities—

	Per cent.
Arsenic	0·330
Iron	0·052
Silver	0·006
Sulphur	0·720

H. B.

On the Treatment of Copper in the Bessemer Converter, by the Manhès Process.

(Bulletin de la Société de l'Industrie Minérale, vol. xiv., 1885, p. 607.)

As the reduction of copper from the ore requires a greater consumption of fuel than any other metal, it can only be carried on where fuel is cheap, and where ore of moderate richness can be easily obtained, as at Swansea and Liverpool. Poor ores can only be manipulated under such conditions as prevail in parts of Sweden and the Ural district, where both wood and labour are very cheap ; while the reduction in the price of merchant copper from 2,300 to 1,100 francs (£90 to £43) per ton, in consequence of severe American competition, has almost ruined the trade in Germany, Sweden, Russia, and especially in France.

During the last thirty or forty years the ordinary methods employed in England, known as the French (fully described by Leplay) and the Swedish (described by Rivot and Percy), have been practised with very little modification, except in the increased size of the furnaces, and the utilisation of the waste products of calcining for the manufacture of sulphuric acid, while with poor ores the consumption of fuel is still as high as from 15 to 18 tons per ton of copper produced.

The success of the Bessemer process, as applied to steel-making, raised the question whether the same process could not be advantageously applied to copper. In each case the ore is first reduced in the blast-furnace, to pig-iron in the one case, and to "matte" or coarse copper in the other; and if, in the first, the simple action of the blast will eliminate the silicon, manganese, and carbon, why in the second should it not equally eliminate the sulphur and the iron, each being more oxidizable than the copper?

The fact is, that the elements to be oxidized do not exceed 9 to 10 per cent. of the total weight of the pig, whereas the best

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mattes contain only 50 to 60 per cent., and the average samples only 15 to 25 per cent. of copper, while the combustion of silicon and carbon produces far greater heat than that of sulphur and iron; so that although the specific heat of copper is only some two-thirds that of iron, the greatly increased proportion of slag and dross presents a serious difficulty.

In spite of several unsuccessful attempts by other metallurgists, Mr. Pierre Manhès, of Lyons, again attacked the problem in 1880 at his copper-rolling mills at Védènes (Vaucluse), using an ordinary Bessemer converter blown from the bottom. At first all went well. The sulphur was driven off in the form of sulphuric acid, and the iron combined with the argillaceous lining to form a fluid slag. Then, very suddenly, an ebullition occurred which expelled matte and slag together with great violence, while the heavier metallic copper, deprived of its combustible elements, sank to the bottom, solidified under the cooling action of the blast, and choked the twyers, so that the contents of the converter had to be hastily run off before the complete purification of the upper part of the charge, which consisted of a matte less dense, but more fusible, than pure copper.

The experiments were repeated with mattes containing various proportions of iron and copper, but with the same results, so long as the ordinary Bessemer converter was used. Then a modification was adopted, with a ring of twyers some distance above the bottom, with the result that the metallic copper, no longer exposed to the cooling action of the blast, remained liquid during the later stages of the process, while the introduction of a little quartz, combining with the protoxide of iron, hindered the formation of the magnetic oxide and the consequent corrosion of the lining. This answered very well with rich mattes, containing 50 to 60 per cent. of copper, but with poorer qualities the excess of slag formed, which had to be removed even before the process was complete, still presented a difficulty which could only be overcome by repeating the operation; the first stage bringing a poor matte up to a richness of 60 per cent. of copper, the second producing red copper containing only 1 to 1½ per cent. of impurities.

The experiments thus briefly indicated, which were carried on for a year at the Védènes works, showed that the new process was not only possible, but practicable, and a company was formed for carrying it out at the old royal foundry of Eguilles, near Sorgues (Vaucluse), under the management of Mr. Paul David, who had rendered valuable assistance to Mr. Manhès during the whole of the experiments at Védènes. This establishment is now producing some 2,000 tons of copper a year, and the production is rapidly increasing.

The chief difficulty arose from the extreme variation in the richness of the ores, to such an extent that even with the modified converter above described, the purification had often to be completed in two stages, as the height of the belt of twyers most suitable for poor mattes was too far above the level of the bath of

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molten copper to allow the blast to play upon the mat as it gained in richness.

The David and Manhès converter, as now employed, entirely obviates this difficulty. Its form is that of a horizontal cylinder, the orifice for charging and discharging being on one side, with a row of blast-orifices diametrically opposite, and is capable of revolution on its axis, by means of suitable gearing, without interfering with the continuity of the blast, which can consequently be injected at any height from the bottom that may be required at various stages of the process. The charge is 1 ton of matte, and the blast-pressure from 10 to 12 inches of mercury. The temperature rises rapidly, dense white sulphurous vapours being given off, and discharged into a flue leading to a large chimney 164 feet high. After fifteen or twenty minutes, according to the richness of the matte, these vapours clear, and, with a poor matte, the charge must be poured *at once*, or a tumultuous action takes place, and the slag is suddenly and violently ejected. With rich matte there is no difficulty; the copper, of a purity of 98 or 99 per cent., is poured into iron ingot moulds, and the slag, containing 2 or 3 per cent. of copper at most, is run out on to the foundry floor, and returned to the blast furnace.

The entire process, then, consists of but two operations: (1) fusing the ore to obtain a matte, which (2) is run direct into the Manhès converter, where, whatever its richness, and without any additional fuel, it is in a few minutes converted into raw copper.

At first the lining of the converter would only last for ten or twelve charges at most, and was often completely destroyed after seven or eight; now it will often stand for fifteen, sixteen, or even eighteen charges. As in the case of steel-making by the basic process, the principle was well understood, but practical success was only achieved by close attention to matters of detail, often apparently unimportant.

Not only pure mattes, but those containing arsenic, antimony, lead, tin, zinc, &c., are successfully treated, and even old scrap brass, bronze, copper, &c., but neither this process nor any other now in use will completely eliminate nickel and bismuth. The ease with which the copper can be worked and rolled shows conclusively that the specially injurious elements, arsenic and antimony, have been satisfactorily dealt with.

The composition of the copper produced is exceedingly regular, seldom varying from the following proportions:—

	Per cent.
Pure copper	98·5 to 98·8
Sulphur	0·9 „ 0·8
Iron	0·6 „ 0·4
	100

while ordinary Chili raw copper contains at least 4 per cent. of impurities.

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The total consumption of fuel per ton of copper produced is:—

	Kilog.
Motive power	1,000
Fusion of ore	3,000
Refining	700
	4,700

or less than 5 tons as compared with 13 to 16 tons per ton of copper by the English method. With coke at 35 francs (27*s.* 4*d.*) per ton, the expense for fuel is only 140 to 150 francs (£5 9*s.* 6*d.* to £5 17*s.* 3*d.*) per ton of copper; in England, with fuel at less than half the price, it is from 320 to 350 francs (£12 10*s.* 3*d.* to £13 3*s.* 9*d.*); and although many of the operations, such as manipulating the apparatus, breaking the ore, raising it to the furnace, &c., are at present done by hand, the cost of labour does not exceed 40 francs (£1 11*s.* 3*d.*) per ton of copper, as against 65 francs (£2 10*s.* 10*d.*) in England.

As the Manhès process takes the place of some six, eight, or ten successive operations, and the ore may in fact be converted into raw copper the same day that it is received, the necessity for holding heavy stocks of material in successive stages is done away with.

In 1884 Mr. Manhès successfully applied the same process to the production of nickel.

The Paper is accompanied by a Plate illustrating the modified Bessemer and the Manhès converters.

W. S. H.

On the Passage of Copper through a Gaseous Medium, and on the direct combination of Copper and Nitrogen.

By R. Blondlot.

(Comptes rendus de l'Académie des Sciences, vol. cii., 1886, p. 210.)

The following phenomenon was observed on making some experiments on the passage of electricity through incandescent gases. A disk of copper and one of platinum 30 millimetres in diameter were fixed parallel to each other, and from 3 to 4 millimetres apart, by two columns of platinum, and enclosed in a bell of glazed porcelain open at the bottom. By means of a gas-furnace at the top the bell was raised to a bright red heat, which was maintained for about three hours, no electric current being passed. The surface of the platinum facing the copper was then found to be considerably changed, being coated with a crust about $\frac{1}{10}$ millimetre thick resembling graphite. This, when scraped off and examined, proved to be soluble in hot though not in cold nitric acid, leaving a residue in the form of a black powder. The dissolved portion contained a considerable quantity of copper (only traces

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having been present in the original platinum), while the residue was a very active platinum black. Therefore there appeared to be evidence of the formation of a chemical compound of the two metals at a distance, the copper having passed through the space between the two disks. The experiment was often repeated with the same result.

As the copper was strongly oxidized in the experiment, it was repeated in pure nitrogen, the disks being placed at the ends of platinum rods, at the proper distance apart, in the centre of a glazed porcelain tube, which was heated to redness for five hours. The same result was obtained, the copper disk being roughened but remaining bright, the interior crystalline structure being revealed by the removal of the outer face. It was evident, therefore, that oxygen played no part in the action, and the repetition of the experiment in hydrogen with a negative result showed that nitrogen was an essential factor, the copper probably forming a direct combination with that gas which then, either wholly or in part, fixed itself upon the platinum.

The Author has further proved that the combination is dissociable at very high temperatures, for having had occasion to heat a platinum column so coated, he found that the deposit travelled from the hottest and accumulated at the coldest part. The fact of the dissociation is further proved by the following fact. When the operation is performed in an open bell as first described, the deposit only takes place on the front of the platinum disk, and not on the back; while the platinum supports, on the other hand, are free from the deposit at the top, but abundantly covered at the bottom. This is explained by the distribution of heat on the bell, the temperature, and with it the tension of dissociation, being very considerable above, so that the compound cannot form except in the immediate vicinity of the source of copper, while lower down the temperature is less, and the compound forms more readily.

H. B.

Fusion by Electricity. By A. M. Tanner.

(La Lumière Électrique, vol. xix., 1886, pp. 350–352.)

The Author refers to the first electrical fusion by Sir H. Davy, and instances Grove's furnace,[1] which consisted of a carbon crucible, containing the metal to be fused and immersed in a bath of mercury, the crucible being covered with a carbon plate. This plate communicated with one of the terminals of the battery; the mercury with the other. Du Moncel mentions[2] the use of calorific effects produced by electric current for fusion of platinum, iridium,

[1] Gilbert's Annalen der Physik, vol. lv. p. 155.
[2] Applications de l'Électricité, vol. iii. p. 316, 1857.

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osmium, &c., the operation taking place in a crucible of retort carbon. Pichon patented in France (1853) a furnace,[1] in which the mineral, mixed with fragments of carbon, was caused to fall between carbon electrodes bearing an electric arc and placed in the interior of a large muffle. The molten metal was received into a reservoir heated to the temperature of its fusion. Joule, in connection with (then) Professor Thomson, instituted a large number of experiments as to the electrical fusion of metals. In an article published in 1856,[2] Sir William Thomson declared that by passing a strong current across a bundle of iron wires, surrounded by wood-charcoal, he had obtained fusion of part of the bundle. Joule later made a series of analogous experiments, fusing wires within glass tubes surrounded by wood-charcoal placed in a trough. Sir W. Siemens, however, takes the practical honours with his now well-known furnace of 1878. In 1878, also, Fox obtained fusion heat by passing a current through a wall of mixed carbon and refractory clay surrounding a crucible. In 1881 the Compagnie Générale Belge de Lumière Électrique constructed an electric crucible, using as refractory substance a mixture of oxide of iron and magnesia. The outcome of these successive steps is the Cowles process.[3]

P. H.

On the Electrolytic Treatment of Argentiferous Zinc.

(Dingler's Polytechnische Journal, vol. cclix., 1886, p. 291.)

At Friedrichshütte, in Silesia, the following modification of the treatment of the zinc, lead and silver alloy obtained in desilverizing lead by Parkes's process has been introduced. The zinc-skimmings, averaging lead 90, zinc 8–10, and silver 0·5–2·0 per cent., are granulated and placed in a layer at the bottom of a lead-lined tub filled with a solution of sulphate of zinc, forming the electrolyte. The positive electrode is formed by the lead plate at the bottom, while the cathode is a zinc plate suspended vertically in the liquid. When a current of sufficient strength is passed through the apparatus zinc is deposited on the cathode, a corresponding quantity of the metal being dissolved from the granulated alloy, so that the strength of the solution is kept approximately constant. The decomposition goes on until the residual alloy, which may be about one-half of the regular quantity, is protected by the excess of lead from further action, when it is washed, dried, and subjected to liquation. By the latter operation lead is separated of sufficient purity to be passed at once to the refinery, while the residue contains zinc and lead in about the same proportion as the original

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[1] Dingler's Journal, vol. cxxxi. p. 415.
[2] Proceedings Literary and Philosophical Society of Manchester, vol. xiv. p. 49.
[3] Min. Proc. Inst. C.E., vol. lxxxiii. p. 510.

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zinc-skimmings, and is of the same fine-grained character. This is subjected to the electrolytic process a second time, followed by subsequent liquation. By these alternate operations, zinc, lead, and silver are obtained in the metallic state, instead of the mixed oxides of lead and zinc formerly produced in considerable quantity, which could only be made marketable with difficulty.

H. B.

On the Electrical Resistance of Copper at Extremely Low Temperatures. By SIGMUND V. WROBLEWSKI.

(Centralblatt für Elektrotechnik, vol. vii., 1886, p. 677.)

Clausius has drawn attention[1] to the similarity in the numerical value of the temperature coefficient of the electrical resistance of pure metals, to that of expansion of the more permanent gases, with the remark that this points to the conclusion that at absolute zero metals would possess infinite conducting power.

The Author has measured the resistance of a diminutive bobbin of fine copper-wire, of at least 98 per cent. conductivity, when subjected to the various low temperatures obtainable in the reservoir of his apparatus used for the liquefaction of gases. The temperatures at which the observations were made are given as follows:—

	Centigrade.
Boiling-point of water	100°
Melting-point of ice	0°
Boiling-point of ethyl under atmospheric pressure . .	−103°
Critical temperature of nitrogen . . . . . about	−146°
Boiling-point of nitrogen at atmospheric pressure . .	−193°
Just above the solidifying-point of nitrogen . . about	−201°

The results recorded of four series of observations showed that there was no permanent alteration in the resistance due to the extreme cold, and that the electrical resistance diminished at a greater rate than the temperature; so that the resistance would actually vanish before absolute zero was reached.

F. J.

Faraday's Law. By J. MOUTIER.

(La Lumière Électrique, vol. xix., 1886, p. 145.)

The law of electro-chemical equivalents, discovered by Faraday, seems to have escaped all theoretical interpretation; it is expedient to see whether the consideration of free energy can furnish an explanation of this law.

When a circuit includes several electromotive forces, the quantity

[1] "Poggendorf's Annalen," vol. civ. p. 650.

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of heat disengaged in the circuit during the unit of time, or the voltaic heat, is, according to Joule's law,

$$A \Sigma E i,$$

where A is the calorific equivalent of work, i the current, and E an electromotive force, which acting alone will produce currents of the same direction as those traversing the circuit, counting negatively the electromotive forces of electrolytes that act in an opposite direction. The heat disengaged in every isothermic and irreversible transformation is compounded of two parts, compensated heat and non-compensated heat, the latter being positive. Helmholtz has shown that voltaic heat is equal to the non-compensated heat accompanying the chemical reaction in the voltaic couple. To the non-compensated heat corresponds a non-compensated work, which is the variation, with changed sign, of the function designated by Helmholtz free energy. If L be the diminution of free energy of a voltaic couple at the end of a certain time, E the electromotive force of the couple, Q the quantity of electricity put in play in the same time, according to Helmholtz's theory

$$L = E Q.$$

The diminution of free energy is a product of two factors, the electromotive force which is independent of the dimensions of the couple, and the quantity modified by the couple. When the same chemical reaction occurs in the couple, the diminution of free energy is proportional to the weight of one of the elements participating in the reaction. When the circuit includes an electrolyte, the weight of this electrolyte decomposed in unit time is in constant ratio with the weight of metal dissolved in each of the voltaic couples. The value of this constant ratio is determined by Faraday's law; and to ascertain whether it is possible to find an explanation of this law in the consideration of free energy, the Author examines the solution of zinc in sulphuric acid, supposing (1) a zinc-acid couple placed alone in circuit, (2) this couple and a voltameter, the electrolysis of hydrochloric acid, and the decomposition of chloride of copper. From these examples it seems permissible to hope that the consideration of free energy introduced by Helmholtz may furnish an interpretation of the law.

P. H.

The Deprez-d'Arsonval Galvanometer. By — LEDEBOER.

(Comptes rendus de l'Académie des Sciences, vol. cii., 1886, p. 504.)

To find the impulsion δ due to the passage of the quantity of electricity q (passing through the wire of the moving frame), the Author deduces the simplified formula

$$\delta = \frac{a}{i} \frac{\pi}{T} \frac{1}{e} q,$$

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as realized by the discharge of a condenser, the galvanometer being so shunted that the total resistance of the circuit is $R = \frac{1}{2}\frac{\pi^2}{T^2}\frac{a^2}{i^2}\Sigma m r^2$. $\frac{a}{i}$ = constant of galvanometer; $\Sigma m r^2$ = moment of inertia of moving frame; T = duration of a simple oscillation on open circuit.

When the shunt is removed, the discharge of the same condenser should give a deflection

$$\delta' = \frac{a}{i}\frac{\pi}{T}q,$$

and the ratio of $\frac{\delta'}{\delta} = e = 2{\cdot}72$, the base of Napierian logarithms. The experimental verification gave 2·52 for a galvanometer with permanent magnet, and 2·68 for a galvanometer with electro-magnet. The constancy of the magnetic field (to defect of which deviation from theoretical value is due) is greater with the electro-magnetic field.

P. H.

Experimental Verification of Verdet's Law.

By A. Cornu and A. Potier.

(Comptes rendus de l'Académie des Sciences, vol. cii., 1886, p. 385.)

The existence of a particular double refraction presented by isotropic bodies placed in a magnetic field, following a direction normal to the lines of force, has been established theoretically by one of the Authors, by supposing the law of Verdet exact. As this law has been verified only in directions where the rotary magnetic power is still notable, and as the strict exactness of this law has been contested, it appeared necessary to push the verification as far as possible, that is to directions where the rotation is annulled. The end of this verification, besides the interest that attaches to a physical law, was to demonstrate implicitly that the two sheets of the surface of the magnetic wave intersect at a finite angle, and have a strictly spherical form. Faraday, having discovered that the magnetic rotation ω is annulled in the direction normal to the lines of force, and changes sign when the luminous pencil passes from one side to the other of this direction, it results that the angle ω is necessarily an odd function of the angle β, made by the pencil with the direction normal to the lines of force: $\omega = b\beta + c\beta^2 + \ldots$ The question to be resolved is to know if the term in β really exists, its existence entailing the consequence that the two sheets intersect under a finite angle proportional to b. If Verdet's law is exact this term exists, for its expression may be written—

$$\omega = a\cos\left(\frac{\pi}{2} - \beta\right) = a\sin\beta = a\left(\beta - \frac{\beta^3}{6} + \ldots\right).$$

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This identification shows that the coefficient b is no other than a in the cosine law; it is to the verification of this equality that the researches tend. The experimental difficulties included the realization of a sufficiently intense magnetic field, and of sensible uniformity; two nearly contradictory conditions, which were met by the use of an intense electric source (Gramme dynamo and thirty accumulators), and an electro-magnetic apparatus of small dimensions. The Authors describe the form of magnet and apparatus employed, and in conclusion state that Verdet's law is satisfied with all the precision that the apparatus would bear, the small divergences being attributable to defect in uniformity of the magnetic field.

P. H.

The Influence of Storms on Subterranean Telegraph Lines.

By — BLAVIER.

(Journal de Physique, vol. iv., 1885, p. 553.)

Electric discharges have been frequently observed at telegraph offices where the stations are connected by means of subterranean wires, and a storm has taken place somewhere between them. For example, during a violent storm which occurred on the 9th of March, 1885, midway between the towns of Belfort and Besançon, sparks were observed at the telegraph instruments in these two stations, although in the towns themselves there was not the slightest indication of atmospheric disturbance. The Author is of opinion that the phenomenon may be explained either as an effect of electrodynamic or electrostatic induction. If the cable is buried at only a small depth in soil of low conducting power, the metallic guard is more or less charged under the influence of magnetic storms, although the conducting wire is quite free from charge. At the moment of a flash occurring, the metallic guard is suddenly discharged either partially or altogether, and passes to earth in the two opposite directions.

There are thus developed in the conducting wire two induced currents in opposite directions, whose difference alone influences the instruments at the extreme stations. This effect must be very small, as, the electricity passing rapidly into the soil, the induction can only be very limited. A second effect results from the circumstance that the discharge of the metallic guard not being instantaneous its electric potential diminishes suddenly. The free electricity reacts on the conducting wire, which is suddenly charged with electricity of an opposite sign through the points in communication with the earth, that is to say, through the telegraphic apparatus. The exterior charge, in passing away, produces in the conductor an electric movement opposed to the former, which it follows so rapidly, that it generally counteracts it, and thus it happens that it is only very seldom that the phenomena referred to are observed.

E. F. B.

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On the Deflection of Equipotential Lines and the Variation of the Resistance of Bismuth in a Magnetic Field. By — LEDUC.

(Comptes rendus de l'Académie des Sciences, vol. cii., 1886, p. 358.)

The variations of resistance are represented by a parabolic formula of the form $\rho_M = \rho_o(1 + a\,M + b\,M^2 - c\,M^3)$; $\rho_t = \rho_o(1 - pt + qt^2)$, where t is temperature and M intensity of magnetic field. The numerical values of the coefficients are given. In the study of Hall's phenomenon in the plate of bismuth under experiment, the Author found that the values of $\frac{e}{I}$ (ratio of potential-difference to current) are related to the magnetic intensity M as $\frac{e}{I} = \mu\,M\,(1 - a\,M + \beta\,M^2)$; that the deflection of an equipotential line is absolutely independent of the quantity of current traversing the plate; and that the deflection was at a maximum for a temperature of 49°.

As the resistance of bismuth increases more than 15 per cent. in a magnetic field of 10,000 C. G. S. units, the Author proposes to utilize this variation, once ascertained, to measure magnetic intensities. This increase is due partly to the deflection of equipotential lines; the resistance ought to be multiplied by $\frac{1}{\cos D}$ (D, deflection of equipotential line). But it is evident that this coefficient does not exceed 1·005. It is therefore necessary to seek explanation of this phenomenon in change of structure of the metal.

P. H.

Electrolysis of Salts. By A. RENARD.

(Comptes rendus de l'Académie des Sciences, vol. cii., 1886, p. 361.)

Influence of Temperature.—The experiments were made on solutions containing, per cent., equivalent atomic weights of metal varying from $\frac{2}{10,000}$ to $\frac{64}{10,000}$. The electrode surface was 226 square millimetres, distance of electrodes apart 0·02 metre, electromotive force of current 3·65 volts. The Author found that in dilute solutions the amount of metal precipitated increased proportionally to the temperature, according to the formula

$$P_t = P_o\,(1 + K\,t),$$

in which K increases as the concentration diminishes. When the solutions are so diluted that the quantity of metal precipitated at the same temperature is proportional to the concentration, K attains a maximum value of about 0·052.

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Influence of Distance of Electrodes.—When the electrode distance is successively doubled, the quantity of metal precipitated decreases in geometric progression. For distances greater than 0·32 metre the law is no longer applicable.

Influence of Electrode Surface.—The solutions employed were of nitrate and of sulphate of copper; the electrodes discs of copper of 0·1, 0·2, and 0·3 metre diameter at a fixed distance of 0·02 metre. It is concluded that when the two electrodes are identical, the quantities of metal precipitated are proportional to their diameters or to the square root of the surface.

P. H.

The Coefficient of Expansion of Gases and Vapours under Constant Pressure. By C. ANTOINE.[1]

(Paper destined for the French Académie des Sciences.)

If a special zero is assumed for each gas or vapour other than the ordinary absolute zero—273° Centigrade—it is found that by counting temperatures from that zero the laws for each gas or vapour can be very simply expressed.

The pressure at saturation is expressed by a formula of the form

$$F = A\,T^{5\cdot5} = A\,(B + t)^{5\cdot5},$$

where T is the temperature reckoned from the special zero, and t is the temperature Centigrade. Experiment shows that the pressure in atmospheres of the following gases is very approximately given by the equations:—

Oxygen	$P = [0\cdot012822\,(272 + t)]^{5\cdot5}$
Nitrogen	$P = [0\cdot01874\,(248 + t)]^{5\cdot5}$
Air	$P = [0\cdot018336\,(246 + t)]^{5\cdot5}$
Carbonic anhydride .	$P = [0\cdot009183\,(208 + t)]^{5\cdot5}$
Steam	$P = [0\cdot0064517\,(55 + t)]^{5\cdot5}$.

Similarly, counting the temperatures T from the special zero, the volume at saturation is expressed by the general formula—

$$V = \frac{c}{T^{5\cdot166}} = \frac{c}{(B + t)^{5\cdot166}}.$$

Hence,

$$PV = E\,T^{0\cdot333} = E\,(B + t)^{\frac{1}{3}},$$

c, B and E, being constants for each gas or vapour.

Starting from the temperature of saturation, if the vapour is superheated, the volume of the vapour, and consequently the product PV, increases, and it may be provisionally assumed that PV increases proportionally to the increase of temperature.

---

[1] The original is in the library Inst. C.E.

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For simplicity, let θ, the temperature of saturation, be reckoned from the special zero. For a temperature T of the superheated vapour—

$$P V = E \theta^{\frac{1}{3}} + D (T - \theta);$$

or if $\frac{E}{D} = K$,

$$P V = (K \theta^{\frac{1}{3}} + T - \theta) D.$$

The ratio of two values of P V is—

$$\frac{P V}{P' V'} = \frac{K \theta^{\frac{1}{3}} + T - \theta}{K \theta'^{\frac{1}{3}} + T' - \theta'} = \frac{K \theta^{\frac{1}{3}} - \theta + B + t}{K \theta'^{\frac{1}{3}} - \theta' + B + t'},$$

which may be written in the form—

$$\frac{P V}{P' V'} = \frac{\beta + t}{\beta' + t'}.$$

But if α is the ordinary coefficient of expansion of gas or vapour, we have from Gay Lussac's law, V and V' being the volumes at different temperatures t and t' Centigrade, but at the same pressure

$$\frac{V}{V'} = \frac{1 + \alpha t}{1 + \alpha t'},$$

which is the same expression as before if $\beta = \frac{1}{\alpha}$. Consequently, if the coefficient of dilatation is known for any one pressure, β and K can be determined, and thence the coefficient of expansion at any temperature.

Numerical application to steam.—Twelve years since the Author recognized that if P is the pressure, V the volume, of saturated steam, then approximately $P V^{1 \cdot 1}$ = constant. Since then Professor Zeuner has given the better expression—

$$P V^{1 \cdot 0646} = \text{constant}.$$

For studying the volumes of saturated steam, there are experiments by Fairbairn and Tate, and by Hirn. The former agree well with the expression—

$$V = 0 \cdot 0657 \left(\frac{1{,}000}{\theta^{1 \cdot 06}}\right)^5.$$

The experiments of Hirn agree better with the expression—

$$V = 0 \cdot 03412 \left(\frac{1{,}000}{\theta^{1 \cdot 0333}}\right)^5,$$

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where θ is the temperature of saturation (reckoned from $-55°$ Centigrade). The latter expression is equivalent to—

$$V = \frac{c}{\theta^{5 \cdot 1666}}.$$

Hence

$$PV = \frac{A\, c\, \theta^{5 \cdot 5}}{\theta^{5 \cdot 1666}} = E\, \theta^{\frac{1}{3}},$$

or $P V^{1 \cdot 0647} =$ constant, thus reproducing the formula of Professor Zeuner.

It is generally admitted that at one atmosphere the coefficient of expansion of steam is the same as for air. The temperature θ for one atmosphere is 155° reckoned from the special zero for steam. Hence $\beta = 273$, $K = 69 \cdot 44$. Consequently for any other temperature of saturation θ, the value of β, which is the reciprocal of the coefficient of expansion, is—

$$\beta = K\, \theta^{\frac{1}{3}} + 55 - \theta.$$

Taking the volume of steam in litres at one atmosphere as 1645, we get $E = 306 \cdot 32$. So that finally—

$$PV = 4 \cdot 4114\, (\beta + t).$$

The Author gives Tables showing the agreement of these formulas with Fairbairn and Tate's, and Hirn's experiments.

W. C. U.

On the Influence of the Composition of the Glass on the Readings of Thermometers. By H. F. WIEBE.

(Sitzungsberichte der Königlich. Preussischen Akademie der Wissenschaften, Berlin, 1885, p. 1021.)

The Author continues in this Paper his thermometric researches previously published in the "Sitzungsberichte," 1884, p. 843. He has made use of a glass, easily manipulated in the flame, and much less subject to the thermic effects than the glass employed for thermometers during late years in Germany, and in a still higher degree than the glass generally used elsewhere. This kind of glass, denoted by XVI., has the following composition:—

XVI.

Silicic acid	67·5
Soda	14·0
Zinc oxide	7·0
Lime	7·0
Alumina	2·5
Boric acid	2·0

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Two other types, less easily worked in the flame, though having equally favourable thermic properties, have compositions as under:—

XIV.		XVIII.	
Silicic acid	69	Silicic acid	52
Soda	14	Potash	9
Zinc oxide	7	Zinc oxide	30
Lime	7	Boric acid	9
Alumina	1		
Boric acid	2		

All three kinds of glass, which for convenience may be distinguished as "Jena glass," have the following important characteristics: 1st. The depression constant, *i.e.*, the after-effect of heating to 100° on the readings of the thermometer, only amounts to 0·05°. 2. The gradual rise of the freezing-point, due to the contraction of the glass, is within much narrower limits than have been observed after the lapse of the same periods of time with thermometers of English and French glass. In the Table following it should be noticed that in the first five and last three rows, a short time only had intervened between the construction of the thermometer and the observations, and that the number reduced to the Centigrade scale represented the mean of observations with two or more similarly constructed thermometers. Besides the glass of composition above referred to the Table contains three others, designated by XXII., XVII., XXXI., experimented on for the sake of comparison, and which exhibit the effects in a specially marked degree, and the composition of which, given hereafter, corresponds essentially with the worst Thuringian thermometer glasses recently brought into the market. For the sake of completeness a passable Thuringian glass is added, which has been in use for some years. The last results, however, are not directly comparable with the others, as the thermometers had been constructed six or seven months before the commencement of the observations.

Glass.	Depression for 100°.	Rise of Freezing-Point in Days.																
		4	21	42	49	66	88	135	160	175	200	242	285	317	447	570	1,450	1,600
XIV.	0·05	–	–	–	–	0·02	–	–	–	–	–	0·03	–	0·04	–	–	–	–
XVI.	0·05	–	–	–	–	–	0·04	–	–	–	–	–	0·04	–	–	–	–	–
XVIII.	0·05	–	–	–	–	–	–	–	–	–	0·04	–	0·05	–	–	–	–	–
French	0·07	–	–	–	0·02	0·04	–	–	–	–	0·07	–	–	–	–	–	–	–
English	0·18	–	–	–	–	–	0·10	0·12	–	–	–	–	–	0·15	0·16	–	–	–
Thuringian	0·36	–	–	–	–	–	–	–	–	–	0·25	–	–	–	–	0·38	0·43	0·44
XXII.	1·05	0·05	–	0·09	0·12	–	0·16	–	0·25	0·25	–	–	–	–	–	–	–	–
XVII.	1·06	–	–	0·22	–	0·33	–	–	–	–	–	–	0·43	–	–	–	–	–
XXXI.	1·03	0·24	0·42	–	–	–	–	0·53	–	–	–	–	–	–	–	–	–	–

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These numbers show that the thermometers of Jena glass behave considerably more favourably, not only in respect of the depression but also in respect to the secular rise of the freezing-point, than the best English and French thermometers. Further experiments were made with thermometers constructed according to the annealing process first employed by Welsh at Kew, which show the superiority of the Jena glass to the foreign glass still more clearly :—

Glass.	After Warming during	Rise of the Freezing-Point.
XIV.	7 hours.	0·02°
XVI.	7 ,,	0·01
XVIII.	7 ,,	0·01
French	8 ,,	0·05
English	9 ,,	0·16
XX.	8 ,,	0·15

The reductions of the readings of thermometers of Jena glass to the readings of an air-thermometer are smaller and more regular than the corresponding reductions for thermometers of the ordinary Thuringian glass. On the other hand the reductions of the French thermometers of Tonnelot are undoubtedly on an average somewhat less, whereas the English thermometers deviate very considerably from the results with the French and Jena instruments, and also exhibit a less simple ratio of the reduction values.

With regard to the thermometer of English glass, it should be remarked that the observations given are the mean of those on four thermometers, two of which were made of crystal glass, and the other two being Kew standards. The latter, however, agreed so well with the former that it is probable the composition of the glass was identical.

The composition of the French glass of Tonnelot is as follows :—

Silicic acid	70·96
Soda	12·02
Potash	0·56
Magnesia	0·40
Lime	14·40
Alumina and oxide of iron . . .	1·44
	99·78

showing it to be the same as the ordinary window glass.

The Author proceeds to discuss the influence of potassium and sodium on the thermic effects, and concludes that they ought to entirely replace each other; and that glasses in which the sodium is only partially replaced by potassium or *vice versâ* are unfavourable for thermometric purposes; and the unfavourable qualities increase as the ratio of the two metals approaches equality.

The effect of various compositions of a number of glasses on the depression constant is summarised in the Table following :—

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Kind of Glass.	Silicic Acid.	Soda.	Potash	Oxide of Lead.	Zinc Oxide.	Lime.	Barium.	Oxide of Lithium	Alumina.	Boric Acid.	Depression for 100.°
II.	24	7·0	—	—	—	—	53	—	16·0	—	0·02
V.	54	—	16·0	—	30·0	—	—	—	—	—	0·09
VII.	51	—	—	3·7	27·7	—	—	6·5	1·8	9·3	0·10
IX.	63	15·0	—	10·0	—	8	—	—	—	4·0	0·08
X.	46	8·0	—	—	—	—	40	—	—	6·0	0·09
XI.	65	—	18·0	—	—	—	—	—	5·0	12·0	0·09
XIX.	50	15·0	—	—	20·0	—	15	—	—	—	0·07
XXIII.	57	8·0	—	—	—	20	—	—	10·0	5·0	0·10
XXXI.	66	11·1	16·9	—	—	6	—	—	—	—	1·03
XVII.	69	15·0	10·5	—	—	—	—	—	5·0	—	1·06
XXII.	66	14·0	14·0	—	—	6	—	—	—	—	1·05

E. H.

On Dynamo-Electric Machines of similar Construction.

By René Arnoux.

(L'Électricien, vol. x., 1886, p. 49.)

In a communication to the French Academy of Science, dated the 20th of February, 1882, Marcel Deprez enunciated his "theorem of similitudes" as follows, viz.: If in a dynamo machine *without iron* the dimensions are increased in the ratio K, the density of the current and the angular velocity remaining constant, the resultant of its elemental force will increase as K^4, and the work per second as K^5. This formula, accepted by different scientists, Professor S. Thompson among them, has been deduced from Ampère's well-known fundamental equation for the static force exerted between two elemental conductors traversed by currents of given intensity. But against its application to the case of dynamo electric machines three objections are raised. Firstly, Ampère's formula was given for static force, and cannot be considered as applicable when the system of conductors is in motion; secondly, the laws which govern the diffusion of heat do not allow of the density of the current being maintained at the same value for all similar machines, for the heat developed by the current increases as K^3, while the radiating surface increases only as K^2, so that the density of the current must be reduced in the ratio $K^{-\frac{1}{2}}$, and in consequence the static force will increase only as K^3; lastly, the angular velocity is limited by the resistance to rupture of the component materials, and for a given limiting value must be reduced in the ration K^{-1}, and therefore the work can increase only as K^3, or, in other words, in proportion to the weight of the machine.

F. J.

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A Danger in connection with Hot-Water Apparatus.

By W. BRÜCKNER, of Vienna.

(Gesundheits-Ingenieur, Jan. 1886, p. 14.)

The Author was instructed, in October 1885, to report upon a defect in the working of a hot-water system, applied to an extensive building in Southern Hungary, the apparatus for which had been constructed by an eminent firm in Pesth, and which at first had given very successful results. Gradually, and after being about a year in use, disturbances arose in the working which appeared to indicate a partial and, in some cases, a complete stoppage of the circulation. It was found upon careful investigation that in each of the coils, and exclusively in the uppermost ring, a copious deposit had taken place of some solid substance, which in certain places had led to a complete stoppage of the pipes. These deposits extended over a section of the pipe about 1 metre in length and decreased gradually in thickness from the centre towards either end. The pipes were properly fitted with expansion valves and were from time to time supplied with extra water to replace that lost by evaporation. This water, as also that used in filling the apparatus in the first instance, was taken from a spring. It was quite clear when drawn, but shewed, after standing for about two hours, a slight deposit and had a brackish taste. This water gave the following results on analysis :—

	Per Litre.
Lime	0·36523 grams.
Magnesium	0·77585 „
Chlorine	1·29680 „
Sulphuric acid	5·61909 „
Carbonic acid	0·38609 „
Alkalies (by difference)	3·47445 „
Total solids	11·91751 „

The lime and magnesium reckoned as sulphates :—

Sulphate of lime	1·24178 grams.
„ magnesia	3·87925 „

The lime and magnesium reckoned as carbonates :—

Carbonate of lime	0·64309 grams.
Sulphate of lime	0·36718 „
„ magnesia	3·87925 „

A mixture of salts of lime and magnesia constituted, therefore, about half the solids present. Specific gravity of the water at 17·5° Centigrade = 1·01037.

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The average composition of the scale or deposit was as follows:—

	In 100 grams.
Chloride of calcium	0·20358 grams.
Sulphate of lime	84·60084 ,,
Carbonate of lime	3·34000 ,,
,, magnesia	6·01958 ,,
Sesquioxide of iron	4·82000 ,,
Water	0·80000 ,,
Total	99·78400 ,,

G. R. R.

On Wire-Drawing without Pickling in Acids. By H. WEDDING.

(Stahl und Eisen, vol. vi., 1886, p. 14.)

In the manufacture of iron-wire the scale formed upon the red-hot metal as it leaves the rolling-mill is removed by soaking or pickling in diluted sulphuric acid, an operation which is repeated after each annealing in the subsequent drawing. In practice the wire is annealed when reduced to 3·8, 2·2 or 2·0, 1·6, 1·1, and 1·0 to 0·7 millimetres diameter. The consumption of sulphuric acid is from 25 to 30 kilograms per ton, of which amount about three-quarters are used for the rolled wire, and the remainder in drawing. The acid is used in the proportion of 1 to 1·2 parts per 100 parts of water. The pickling lasts three hours; the scale is partly dissolved, but the principal effect is simply to loosen it by the evolution of hydrogen from the action of the acid on the metal, so that it falls off during the next drawing. After pickling, the wire is washed with dilute lime-water, and dried on a charcoal fire. Sometimes acid of 50 degrees is used, heated by a steam coil, when the scale is more energetically attacked and the operation is sooner finished. The waste acid liquor in either case contains basic iron salts, ferric hydrate, and gypsum as a muddy deposit, and becomes a serious nuisance when allowed to run to waste in the rivers of the district. Various methods of cleaning wire mechanically, with a view of diminishing the amount of acid required, if not of dispensing with it entirely, have been proposed at different times. The oldest of these, invented by Betz in 1876, consists of a series of rollers with their axes placed at right angles to each other, through which the wire travels, and is bent several times in different planes. This, though moderately successful, has only been very partially adopted, although several modifications of the principle have been made by way of improvement. In Von der Becke's machine fixed slides are substituted for the rollers which scour off the scale. This is effective, but the rubbing is injurious to the wire. An American machine, by Adt, depends upon rollers like those of Betz, with the addition of a rotary motion that allows the whole system of rollers to travel in a spiral path around the axis

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of the wire. This is the most efficient machine of its class, but does not do away entirely with the use of acid, the maximum saving being about 90 per cent. of that required in the ordinary way. Other machines, such as those of Altpeter and Bansen, remove the scale by distorting the section of the wire from a circle to an oval, by means of grooved rollers, whose axes are arranged in planes perpendicular or oblique to each other, as in the universal rolling-mill. The deformation of the section, although objectionable in finished work, is of no consequence when the wire has to be drawn finer; but the scale is not very perfectly removed, the thickness of the residual skin being to that remaining after treatment in Betz's machine as 5 to 3.

The Author has experimented upon a new principle, namely, that of straining the wire, while still hot, up to its elastic limit, 62 to 64 kilograms per square millimetre, when the thicker coating of scale falls off, and the thin inner skin, which under ordinary bending adheres firmly, is loosened. A slight bending-motion, as in Adt's machine, suffices to remove the latter, especially if the wire is suddenly cooled with water at the same time. This, though not dispensing entirely with acid, reduces the consumption to one-third of that required by Betz's machine. This method might probably be used in practice by passing the wire, as it leaves the rolling-mill, under strain through Adt's machine, after scaling it with a water-jet. It would be necessary, however, to use boiling water, as cold water causes a superficial hardening of the metal. In the treatment of drawn wire, after discussing the various methods of annealing and cleaning the annealed wire in use, the Author describes experiments made with a view of determining the minimum temperature at which annealing takes place. For this purpose a wire of 2 millimetres was drawn down to 0·99 millimetres by eight passes through a draw-plate. The ratio of the area of the first groove to that of the original wire was 0·893, and the tractive power required, corresponding to the elastic limit, 63·2 kilograms per square millimetre, with 0·7 per cent. extension. The breaking strain was 79 kilograms, with 0·9 per cent. extension. These figures represent a strain of 166 kilograms and 208 kilograms respectively upon the finished wire, and a dynamometer was placed between the draw-plate and the drawing apparatus, in order that the former limit might not be exceeded. The speed of drawing was 2 metres per minute. The power required under different conditions for drawing a wire of 2·05 millimetres down to 1·83 millimetre was—

	Kilograms.
1. Wire with scale adhering	110 to 200
2. Hard-drawn wire	105
3. Annealed and pickled wire	67

In a second series of experiments, various methods of surface-lubrication, by coating it with salt, copper, and caustic soda, were tried for the same reduction of section, with the following results:—

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	Kilograms.
4. Common salt	110
5. „ „ and oil	110
6. Coppered by blue vitriol solution	100
7. Caustic soda molten	90

In the third series of experiments the question of the temperature actually necessary for annealing was investigated, as it appeared probable that the visible red heat generally used was far too high. For this purpose substances were used whose melting points ranged from 150° to 800° Centigrade. The wire, after taking the temperature of the different baths, was drawn either as nearly as possible at that temperature, or after being cooled, by passing about 2 metres through the air. The fusion of the different substances was effected in cast-iron vessels in a gas furnace. The results are contained in the following Table. Zinc and salt worked most destructively upon the draw-plate.

		°C.	Hot. Kilograms.	Cold. Kilograms.
8.	Paraffin at melting point	150	80	80
	„ burning „	245	60	60
9.	Tin melting „	235	52	80
10.	Lead „ „	235	60	85
11.	Zinc „ „	433	130	150
12.	Common salt „ „	776	70	90

In a final series of experiments, the wire was drawn down to the finished diameter through the remaining holes in the draw-plate. The advantage at first possessed by paraffin as an annealing material was not maintained, the temperature not being sufficiently high to soften the metal after drawing, and finally the lead bath was found to be the only practically useful one.

Number of Holes.

—	2.	3.	4.	5.	6.	7.	8.
	mm.	mm.	mm.	mm.	mm.	mm.	mm.
Reputed diameter	1·65	1·50	1·40	1·30	1·20	1·10	1·00
True „	1·64	1·49	1·39	1·30	1·135	1·06	0·99
	Kilos.	Kilos.	Kilos.	Kilos.	Kilos.	Kilos.	Kilos.
Paraffin 245° C.	20	50	..	..	..	..	..
Lead 234° C.	37	35	37	35	30	25	20

The finished wire from the lead bath was perfectly flexible, and in every way comparable to a well-annealed one. The Author considers that passing the wire through the bath after each drawing was unnecessary, and that probably a smaller number, say once in two passes, would be sufficient.

Some small scattered particles of lead remain adherent to the finished wire, but this protecting cover is cleaned off at the entry

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of the draw-plate. As lead is more readily oxidised than iron when heated, it is probable that the latter may be protected even when the lead covering is not quite continuous.

From the result of his experiments, the Author considers that the lead bath would be worth trying on a practical scale as a substitute for the ordinary method of annealing in pots and pickling with acid. For this purpose it would be necessary to design an apparatus to allow the wire to pass continuously through the different holes of the draw-plate, as in the most approved modern practice of wire-drawing. In the discussion on the Paper, it was pointed out that the method would not be adopted for wire intended for galvanizing, as the lead might prevent the proper adhesion of the molten zinc.

H. B.

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TO THE

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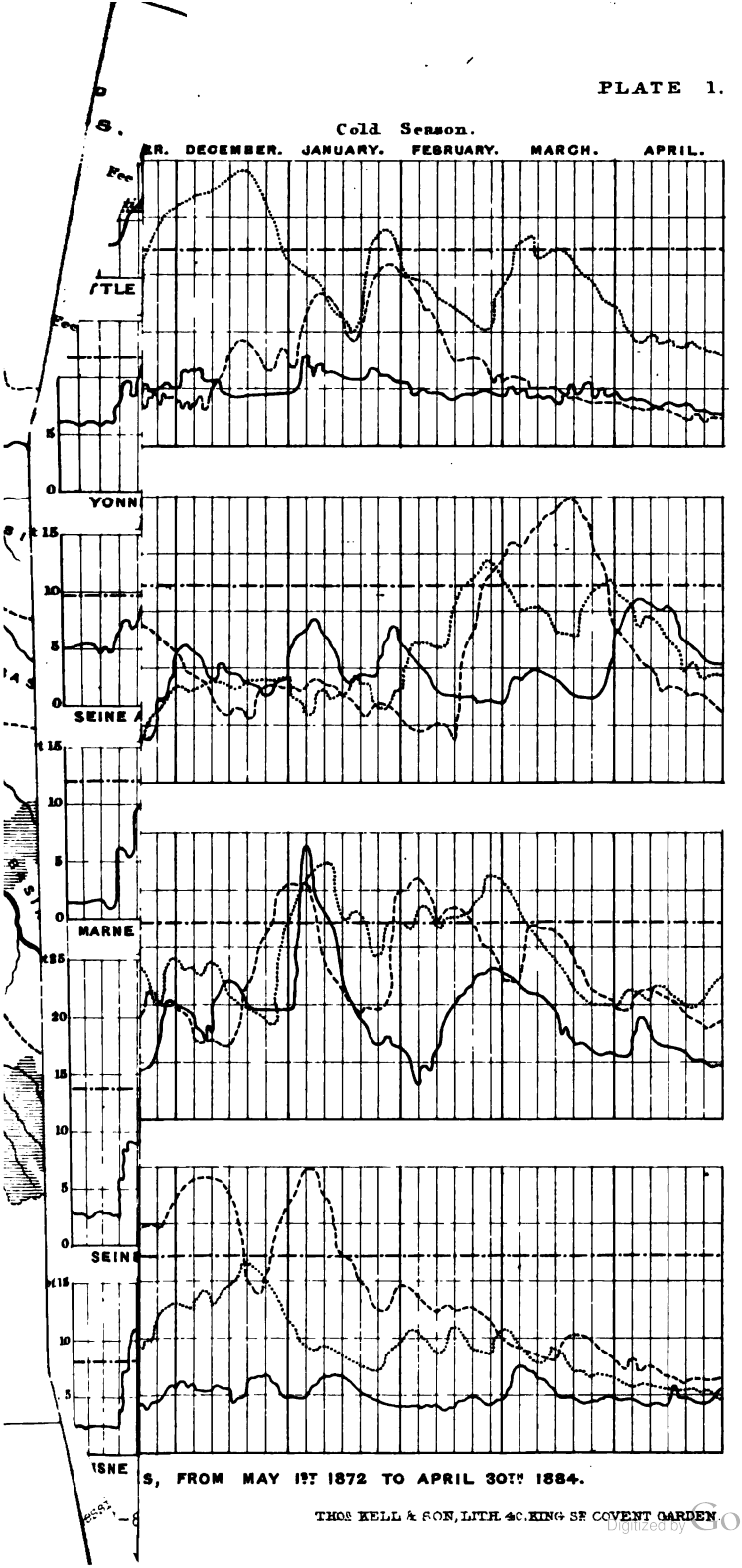

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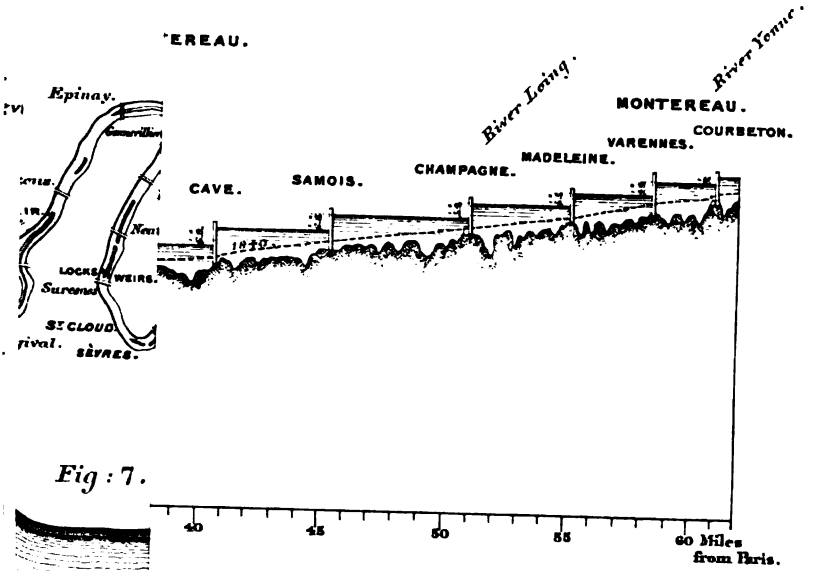
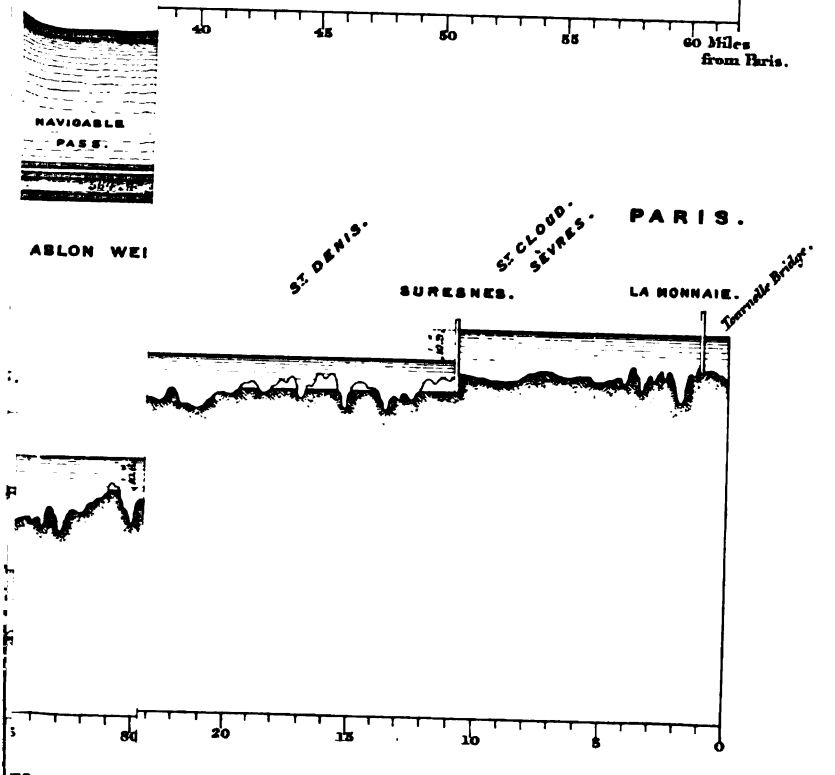


Fig : 7.



THOS KELL & SON, LITH. 40 KING ST COVENT GARDEN.

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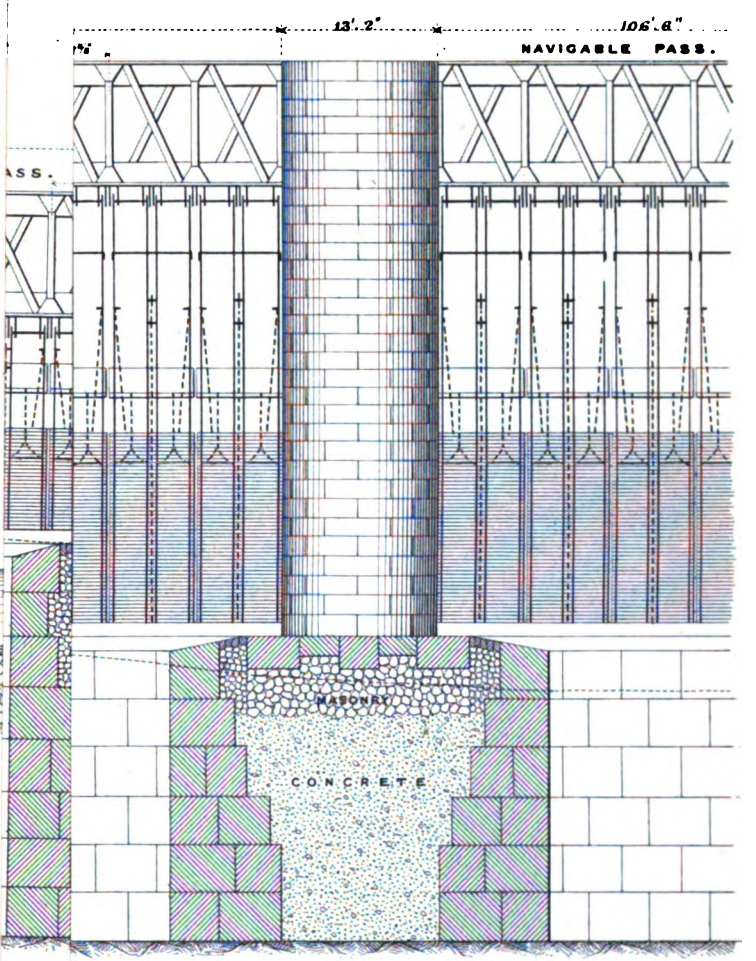
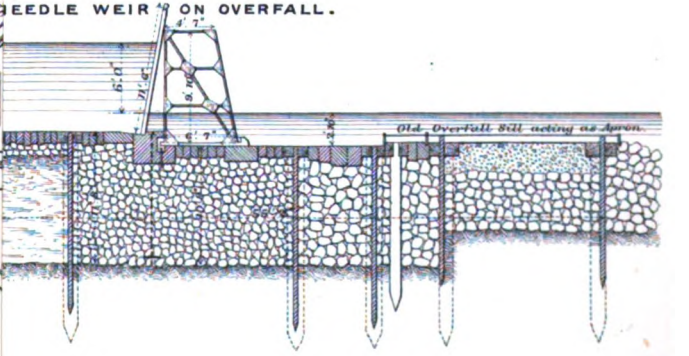


Fig : 9.

NEEDLE WEIR ON OVERFALL.



THOS KELL & SON, LITH. 40, KING ST COVENT GARDEN.

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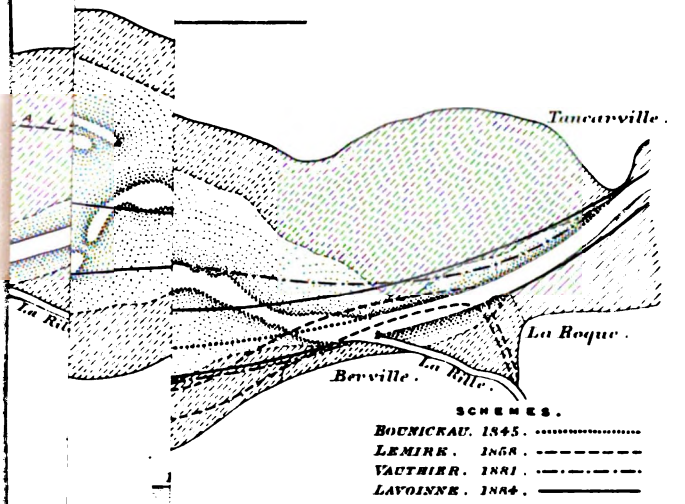
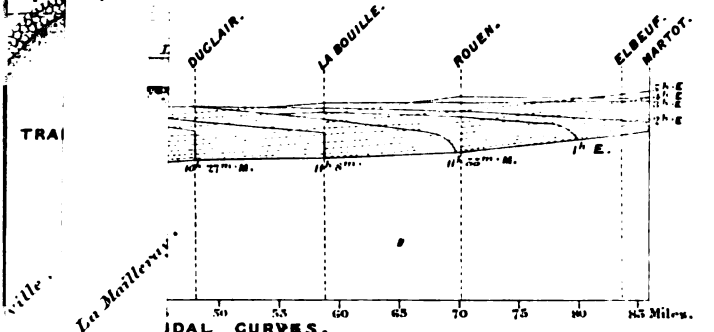
Fig : 9.

SHOWING ESTUARY 1883.

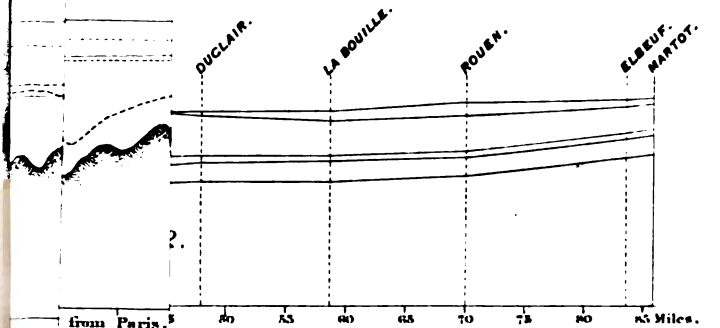
PROLONGATION OF TRAINING WALLS.





IDAL CURVES.

September, 1876.



D LOW WATER.

Saud 26th September, 1876.

THOS KELL & SON LITH 4 KING STREET COVENT GARDEN

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Fig: 9.

E.

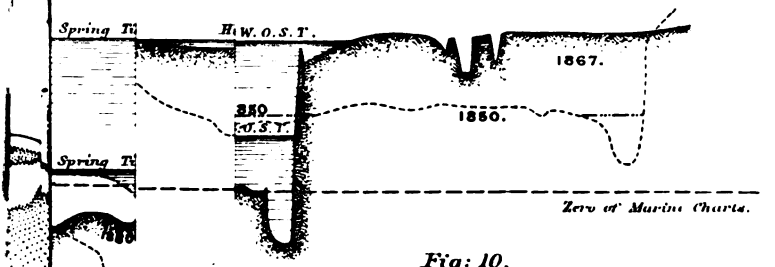

Fig: 10.

F.

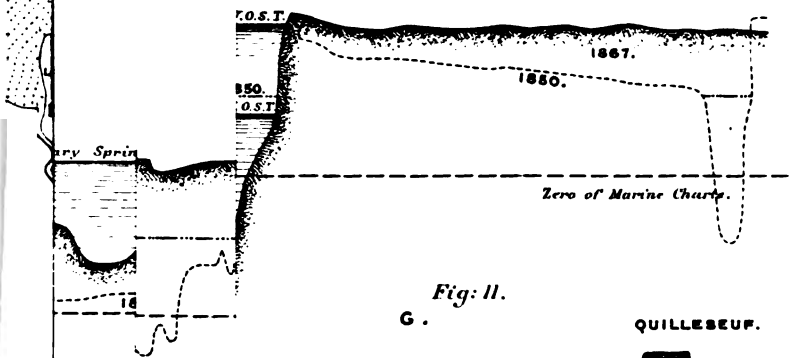

Fig: 11.

G.

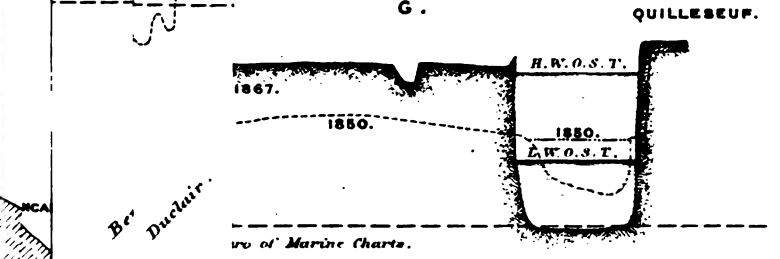

Fig: 12.

H.

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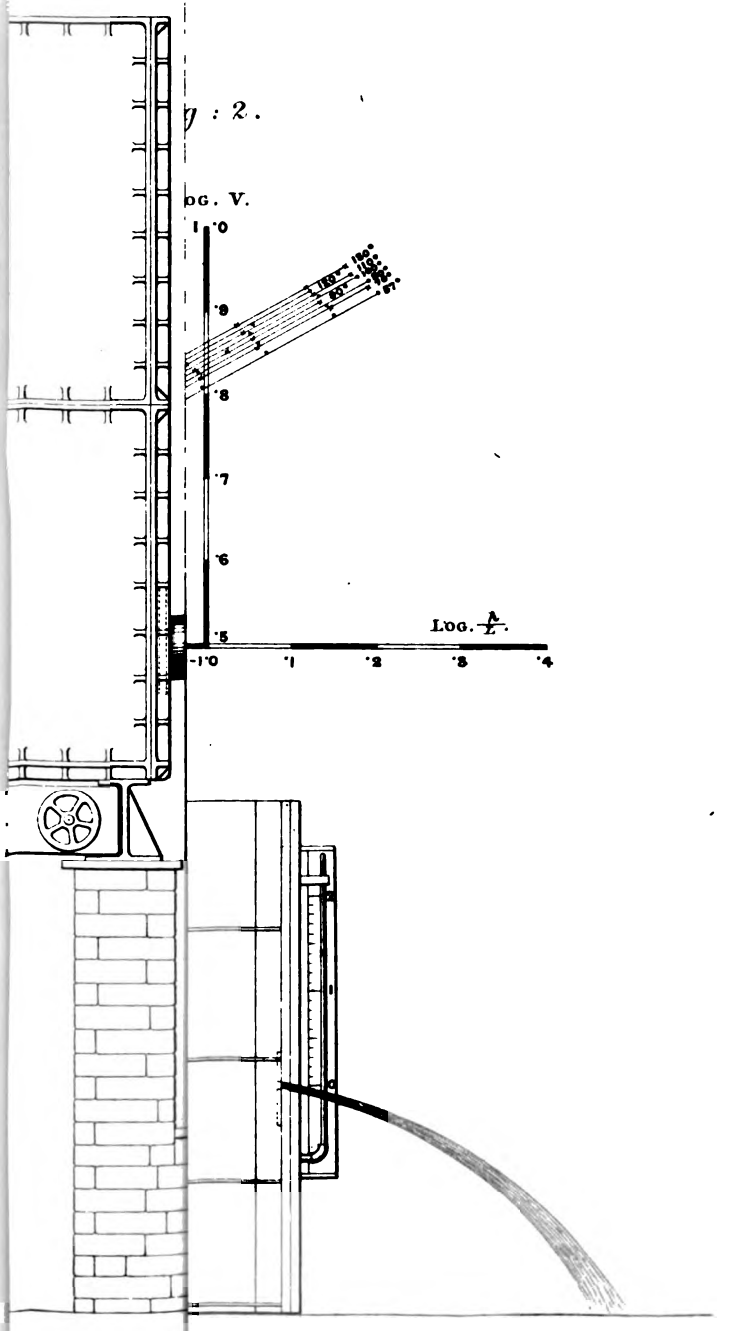

THOS. KELL & SON, LITH. 40 KING ST. COVENT GARDEN.

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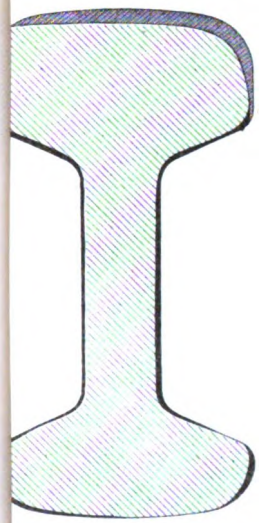
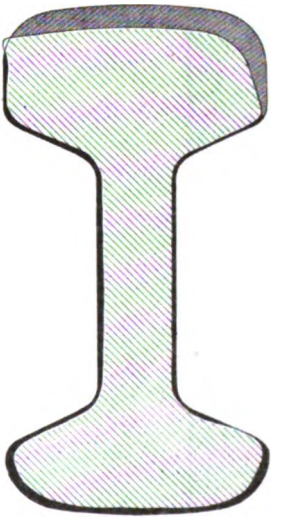
B.



STOCKPORT.

Tonnage 28½ millions.
Life, 12 years.

C.

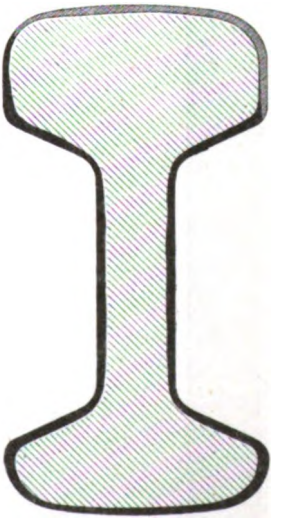


WEAVER JUNCTION.

Tonnage 28½ millions.
Life, 12 years.

E.

F.



DITTON.

Tonnage 13½ millions.
Life, 7 years.

Tonnage (Seldom used.)
Life, 7 years.

Scale ½ full size.

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